PHILOSOPHICAL

TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

LONDON.

FOR THE YEAR MDCCCXI.

PART II.

LONDON,

PRINTED BY W. BULMER AND CO. CLEVELAND-ROW, ST. JAMES'S;
AND SOLD BY G. AND W. NICOL, PALL-MALL, BOOKSELLERS TO HIS MAJESTY,
AND PRINTERS TO THE ROYAL SOCIETY.

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PART I.

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CONTENTS.

XI. *On the Causes which influence the Direction of the Growth of Roots. By* T. A. Knight, *Esq. F. R. S. In a Letter to the Right Hon. Sir* Joseph Banks, *Bart. K. B. P. R. S.* p. 209

XII. *On the Solar Eclipse which is said to have been predicted by* Thales. *By* Francis Baily, *Esq. Communicated by* H. Davy, *Esq. Sec. R. S.* p. 220

XIII. *An Account of the great Derbyshire Denudation. By Mr.* J. Farey, Sen. *In a Letter to the Right Hon. Sir* Joseph Banks, *Bart. K. B. P. R. S.* p. 242

XIV. *An Account of an Appendix to the small Intestines of Birds. By* James Macartney, *Esq. F. R. S.* p. 257

XV. *An Account of a vegetable Wax from Brazil. By* William Thomas Brande, *Esq. F. R. S.* p. 261

XVI. *Astronomical Observations relating to the Construction of the Heavens, arranged for the Purpose of a critical Examination, the Result of which appears to throw some new Light upon the Organization of the celestial Bodies. By* William Herschel, *LL.D. F. R. S.* p. 269

XVII. *Experiments to ascertain the State in which Spirit exists in fermented Liquors: with a Table exhibiting the relative Proportion of pure Alcohol contained in several Kinds of Wine and some other Liquors. By* William Thomas Brande, *Esq. F. R. S.* p. 337

XVIII. *Account of a Lithological Survey of Schehallien, made in order to determine the specific Gravity of the Rocks which compose that Mountain.* By John Playfair, *Esq. F. R. S.* p. 347

XIX. *Observations and Experiments on Vision.* By William Charles Wells, *M. D. F. R. S.* p. 379

Presents received by the Royal Society, from November 1810 *to* July 1811. p. 393

Index. p. 399

PHILOSOPHICAL TRANSACTIONS.

XI. *On the Causes which influence the Direction of the Growth of Roots.* By T. A. Knight, *Esq. F. R. S. In a Letter to the Right Hon. Sir* Joseph Banks, *Bart. K. B. P. R. S.*

Read March 7, 1811.

I HAVE shewn, in a former communication, the effects of centrifugal force upon germinating seeds; from which I have inferred that the radicles are made to descend towards the earth, and the germs, or elongated plumules, to take the opposite direction, by the influence of gravitation; and I believe the facts I have stated to be sufficient to support the inferences I have drawn.* But the fibrous roots of plants, being much less succulent, though not uninfluenced in the directions they take by gravitation, are, to a great extent, obedient to other laws, and are generally found to extend themselves most rapidly, and to the greatest length, in whatever direction the soil is most favourable: whence many naturalists have been disposed to believe that these are guided by some degrees of feeling and perception, analogous to those of animal life.

I shall proceed to state some of the facts upon which this

* Phil. Trans. 1806, 1st Part, page 5.

hypothesis has been founded, and others which have occurred in the course of my own experience, and which are favourable to it; after which I shall endeavour to trace the effects observed to the operation of different causes.

When a tree, which requires much moisture, has sprung up, or been planted, in a dry soil, in the vicinity of water, it has been observed, that much the largest portion of its roots has been directed towards the water; and that when a tree of a different species, and which requires a dry soil, has been placed in a similar situation, it has appeared, in the direction given to its roots, to have avoided the water and moist soil.

A tree growing upon a wall, at some distance from the ground, and consequently ill supplied with food and water, has also been observed to adapt its habits to its situation, and to make very singular and well directed efforts to reach the soil beneath, by means of its roots.* During the period in which it is making such efforts, little addition is made to its branches, and almost the whole powers of the plant appear to be directed to the growth of one or more of its principal roots. To these much is in consequence annually added, and they proceed perpendicularly towards the earth, unless made to deviate by some opposing body: and as soon as the roots have attached themselves to the soil, the branches grow with vigour and rapidity, and the plant assumes the ordinary habits of its species.

DU HAMEL caused two trenches to be made so as to intersect each other at right angles, and a tree to be planted at the point of intersection; and taking up this tree some years afterwards, he found that the roots had almost wholly

* SMITH's Introduction to Botany.

confined themselves to the trenches, in which the soil of the former surface must have been buried.

A trench which was twenty feet long, six wide, and about two deep, was prepared in my garden, in the bottom of which trench was placed a layer, about six inches deep, of very rich mould, incorporated with much fresh vegetable matter. This was covered, eighteen inches deep, with light and poor loam, and upon the bed thus formed, seeds of the common carrot (*Daucus carota*) and parsnip (*Pastinaca sativa*) were sowed. The plants grew feebly till near the end of the summer, when they assumed a very luxuriant growth, grew rapidly till late in the autumn, and till their leaves were injured by frost. The roots were then examined, and were found of an extraordinary length, and in form almost perfectly cylindrical, having scarcely emitted any lateral fibrous roots into the poor soil, whilst the rich mould beneath was filled with them.

In another experiment of the same season, the preceding process was reversed, the rich soil being placed upon the surface, and the poor beneath. The plants here grew very luxuriantly, and acquired a considerable size early in the summer; and when the roots were taken up in the autumn, they were found to have assumed very different forms. The greater part had divided into two or more unequal ramifications, very near the surface of the ground, and those which were not thus divided tapered rapidly to a point at the surface of the poor soil, into which few of their fibrous roots had entered.

In other experiments seeds of almost all the common esculent plants of a garden were so placed that the young plants had an opportunity of selecting either rich, or poor soil;

which was disposed, in almost every possible way, within their reach; and I always found abundant fibrous roots in the rich soil, and comparatively few in the poor.

The following experiment afforded the most remarkable result, and one the least favourable to the hypothesis which I have advanced in a former Paper,* and to the conclusion which I shall now endeavour to support; and therefore I think it necessary to describe it very minutely. Some seeds of the common bean (*Vicia faba*), the plant with which many former experiments were made, were placed upon the surface of the mould in garden pots, in rows which were about four inches distant from each other. A grate, formed of slender bars of wood, was then adapted to the surface of each pot, so as to prevent both the mould and the seeds falling out, in whatever position the pots might be placed; and the bars were so disposed, as not at all to interfere with the radicles of the seeds, when protruding. The pots were then directly inverted; and the seeds were consequently placed beneath the mould; but each seed was so far depressed into the mould, as to be about half covered: by which means each radicle, when first emitted, was in contact with the mould above, and the air below. Water was then introduced through the bottom of the inverted pot, in sufficient quantity to keep the mould moderately moist; and, the pots being suspended from the roof of a forcing house, the seeds soon vegetated.

In former experiments,† wherever the seeds were placed to vegetate at rest, the radicles descended perpendicularly downwards, in whatever direction they were first protruded; but under the preceding circumstances they extended hori-

* Phil. Trans. 1806, page 1. † Ibid.

zontally along the surface of the mould, and in contact with it; and in a few days emitted many fibrous roots upwards into it: just as they would have done, if guided by the instinctive faculties and passions of animal life; and as I concluded before I made the experiment that they would do, under the guidance of much more simple laws, whose mode of operating I shall endeavour to explain.

Whatever be the machinery by which the sap of trees is raised to the extremities of their branches, it is obvious that this machinery is first put into action by the stems and branches, and not by the roots: for the graft or bud, whenever it has become fully united to the stock, wholly regulates the season and temperature, in which the sap is to be put in motion, in perfect independence of the habits of the stock; whether those be late or early. If all the branches of a tree, exclusive of one, be much shaded by contiguous trees,* or other objects, the branch which is exposed to the light attracts to itself a large portion of the ascending sap, which it employs in the formation of leaves and vigorous annual shoots, whilst the shaded branches become languid and unhealthy. The motion of the ascending current of sap appears therefore to be regulated by the ability to employ it in the trunk and branches of the tree; and this current passes up through the alburnum, from which substance the buds and leaves spring. But the sap which gives existence to, and feeds the root, descends through the bark;† and if the operation of light give ability to the exposed branch to attract and employ the ascending or alburnous current of sap, it appears not improbable that the operation of proper food and moisture in the soil,

* Phil. Trans. 1805 and 1809, p. 8. † Phil. Trans. 1809, 1st Part, p. 1.

upon the bark of the root, may give ability to that organ to attract and employ the descending, or cortical current of sap; and if this be the case, an easy explanation of all the preceding phenomena immediately presents itself.

A tree growing upon a wall, and unconnected with the earth, will almost of necessity grow slowly, and as it must be scantily supplied with moisture during the summer, it will rarely produce any other leaves than those which the buds contained, which were formed in the preceding year. Some of the roots of a tree, thus circumstanced, will be less well supplied with moisture than others, and these will be first affected by drought: their points will in consequence become rigid and inexpansible, and they will thence generally cease to elongate at an early period of the summer. The descending current of sap will be then employed in promoting the growth and elongation of those roots only, which are more favourably situated, and those, comparatively with other parts of the tree, will grow rapidly. Gravitation will direct these roots perpendicularly downwards, and the tree will appear to have adopted the wisest and best plan of connecting itself with the ground: and it will really have employed the readiest means of doing so, as effectively as it could have done, if it had possessed all the feelings and instinctive passions and powers of animal life. The subsequent vigorous growth of such a tree is the natural consequence of an improved and more extensive pasture.

When the seeds of the carrot and parsnip, in the experiments I have stated, were placed in a poor superficial soil, but which permitted the roots of the plants to pass readily through it, these were conducted downwards by gravitation; whilst

the plants grew feebly, because they received but little nutriment. The roots were in a situation analogous to that of the stems of trees in a crowded forest; and when the leading fibres of the roots came into contact with the rich mould, they acquired a situation correspondent to that of the leading branches of such trees, which are alone exposed to the light. The form of the roots of the plants was consequently long, slender, and cylindrical, like the stems of such trees. The roots of the one required the actual contact of proper soil and nutriment; and the branches of the other required the actual contact of light, to promote their growth.

When, on the contrary, the seeds of the preceding species of plants were placed in a rich superficial soil, their situation was analogous to that of a tree fully exposed, on every side, to the light, whose branches would be extended, in every direction, immediately above the surface of the ground: and as the fibrous roots of the plants came into contact with the subsoil, which was not well calculated to promote their growth, their situation became analogous to that of shaded branches; and they consequently ceased to extend downwards. The fibrous roots of a tree, under similar circumstances, would have extended along the lower surface of the favourable soil; but after these roots had much increased in bulk, they would be found partly compressed into the subsoil, however poor and unfavourable, provided it contained no ingredients actually noxious. In obedience to similar laws, the roots of an aquatic tree will not extend freely in dry soil, nor those of a tree which requires but little moisture in a wet soil; and on this account the roots of the one will appear to have sought, and those of the other to have avoided, the contiguous water;

though both, in the first period of their growth, pointed their roots alike in every direction.

When the seeds of the bean, in the experiment I have described, were placed to vegetate beneath the mould of an inverted pot, a sufficient quantity of moisture was afforded by the mould to occasion the protrusion of the radicles: but as soon as the under points of these had penetrated through the seed-coats, their surfaces were necessarily exposed to dry air, and were consequently rendered rigid and inexpansible; whilst their upper surfaces, being in contact with the moist mould, remained soft and expansible. If both the upper and lower surfaces of the radicles, at their points, had been equally well supplied with moisture, gravitation would have attracted the sap to the lower sides, where new matter would have been added; and the radicles would have extended perpendicularly downwards, as in former experiments: but the influence of gravitation was, to a great extent, counteracted by the effects of drought upon the lower sides of the radicles, nearly as it was counteracted by centrifugal force, when made to act horizontally.*

As soon as the radicles had acquired sufficient age and maturity, efforts were made by them to emit fibrous roots; when want of proper moisture on the lower sides prevented their being protruded, in any other direction, except upwards. In that direction therefore they were alone emitted, (as I was confident that they would before I began the experiment) and having found proper food and moisture in the pots, they extended themselves upwards through more than half the mould, which these contained.

* Phil. Trans. 1806, p. 6.

This experiment was repeated, and water was so constantly and abundantly given, that every part of the radicles was kept equally wet; and they then became perfectly obedient to gravitation, without being at all influenced by the mould above them.

In other experiments pieces of alum and of the sulphates of iron and copper were placed at small distances perpendicularly beneath the radicles of germinating seeds, of different species, to afford an opportunity of observing whether any efforts would be made by them to avoid poisons; but they did not appear to be at all influenced, except by actual contact of the injurious substances. The growth of their fibrous lateral roots was, however, obviously accelerated, when their points approached any considerable quantity of decomposing vegetable or animal matter: and when the growth of the roots was retarded by want of moisture, the contiguity of water, in the adjoining mould, though not apparently in actual contact with them, operated beneficially: but I had reason to suspect that the growth of roots was, under these circumstances, promoted by actual contact with the detached and fugitive particles of the decomposing body, and of the evaporating water.

The growth and forms assumed by the roots of trees, of every species, are to a great extent, dependent upon the quantity of motion, which their stems and branches receive from winds; for the effects of motion upon the growth of the root, and of the trunk and branches, which I have described in a former memoir, are perfectly similar.* Whatever part of a root is moved and bent by winds, or other causes, an increased deposition of alburnous matter upon that part soon

* Phil. Trans. 1803, p. 7.

takes place, and consequently the roots which immediately adjoin the trunk of an insulated tree, in an exposed situation, become strong and rigid; whilst they diminish rapidly in bulk, as they recede from the trunk, and descend into the ground. By this sudden diminution of the bulk of the roots, the passage of the descending sap, through their bark, is obstructed; and it in consequence generates, and passes into many lateral roots; and these, if the tree be still much agitated by winds, assume a similar form, and consequently divide into many others. A kind of net-work composed of thick and strong roots is thus formed, and the tree is secured from the dangers to which its situation would otherwise expose it.

In a sheltered valley, on the contrary, where a tree is surrounded and protected by others, and is rarely agitated by winds, the roots grow long and slender, like the stem and branches, and comparatively much less of the circulating fluid is expended in the deposition of alburnum beneath the ground; and hence it not unfrequently happens, that a tree, in the most sheltered part of a valley, is uprooted; whilst the exposed and insulated tree, upon the adjoining mountain, remains uninjured by the fury of the storm.

In all the preceding arrangement, the wisdom of nature, and the admirable simplicity of the means it employs, are conspicuously displayed; but I am wholly unable to trace the existence of any thing like sensation or intellect in the plants: and I therefore venture to conclude, that their roots are influenced by the immediate operation and contact of surrounding bodies, and not by any degrees of sensation and passion analogous to those of animal life; and I reject the latter hypothesis, not only because it is founded upon assumptions, which cannot be

granted, but because it is insufficient to explain the preceding phenomena, unless seedling plants be admitted to possess more extensive intellectual powers, than are given to the offspring of the most acute animal. A young wild-duck or partridge, when it first sees the insect upon which nature intends it to feed, instinctively pursues and catches it; but nature has given to the young bird an appropriate organization. The plant, on the contrary, if it could feel and perceive the objects of its wants, and will the possession of them, has still to contrive and form the organ by which these are to be approached. The writers who have contended for the existence of sensation in plants, appear to have been sensible of the preceding and other obstacles, and have all betrayed the weakness of their hypothesis, in adducing a few facts only which are favourable to it, and waving wholly the investigation of all others.

In the description of the preceding experiments, I fear that I have been tediously minute; but, as I have selected a few facts only from a great number, which I could have adduced, I was anxious to give as accurate and distinct a view of those I stated, as possible.

I am, dear Sir,
with great respect,
sincerely yours,

THO. AND. KNIGHT.

Downton, Jan. 15, 1811.

XII. *On the Solar Eclipse which is said to have been predicted by* THALES. *By* Francis Baily, *Esq. Communicated by* H. Davy, *Esq. Sec. R. S.*

Read March 14, 1811.

THERE is probably no fact in ancient history that has given rise to so many discussions, and to such a variety of opinions, as the solar eclipse, which (according to HERODOTUS) is said to have been predicted by THALES; and which, owing to a very singular coincidence, put an end to a furious war that raged between Cyaxares king of Media, and Alyattes king of Lydia.

According to the account given by that celebrated historian, "the contest had continued during five years, with alternate "advantages to each party: in the sixth, there was *a sort of* "*nocturnal combat*. For, after an equal fortune on both sides, "and whilst the two armies were engaging, *the day suddenly* "*became night*. THALES, the Milesian, had predicted this phe- "nomenon to the Ionians: and had ascertained the time of "the year in which it would happen. The Lydians and the "Medes, seeing that the night had thus taken the place of the "day, desisted from the combat; and both parties became "desirous of making peace."—ἐν τοῖσι πολλάκις μὲν οἱ Μῆδοι τοὺς Λυδοὺς ἐνίκησαν, πολλάκις δὲ οἱ Λυδοὶ τοὺς Μήδους· ἐν δὲ καὶ νυκτομαχίην τινὰ ἐποιήσαντο. διαφέρουσι δὲ σφι ἐπὶ ἴσης τὸν πόλεμον, τῷ ἕκτῳ ἔτεϊ συμβολῆς γενομένης, συνήισαν ὥστε τῆς μάχης συνεχωρούσης, τὴν ἡμέρην

ἐξαπίνης νύκτα γενέσθαι. τὴν δὲ μεταλλαγὴν ταύτην τῆς ἡμέρης Θαλῆς ὁ Μιλήσιος τοισι Ἴωσι προηγόρευσε ἔσεσθαι, ὅρον προθέμενος ἐνιαυτὸν τοῦτον, ἐν ᾧ δὴ καὶ ἐγένετο ἡ μεταβολή. οἱ δὲ Λυδοὶ τε καὶ οἱ Μῆδοι ἐπεί τε εἶδον νύκτα ἀντὶ ἡμέρης γινομένην, τῆς μάχης τε ἐπαύσαντο, καὶ μᾶλλόν τι ἔσπευσαν καὶ ἀμφότεροι εἰρήνην ἑωυτοῖσι γενέσθαι. HERODOTUS, Lib. I. §. 74.

The fact is here very clearly (and probably very justly) related: but, unfortunately, there is nothing, either in the statement itself, or in the contiguous passages of the work, that will enable us to determine, with any degree of accuracy, the exact *time* wherein this singular phenomenon took place. And this is the more to be regretted, because the dates of several other events, recorded by the same historian, might be more easily ascertained, if the era of this eclipse were correctly known; but which are now involved in much obscurity.

Deprived of all information from the body of the work itself, chronologists have called in the aid of astronomy to assist them in fixing the date of this remarkable appearance. For it must be evident, that if we could ascertain, by this mean, that in any solar eclipse, which happened about that period, the centre of the moon's shadow passed over the country bordering on the two contesting empires where the battle was probably fought (for HERODOTUS has likewise omitted to mention the *place* where the action occurred), we may reasonably and very fairly conclude, that that eclipse only was the one alluded to by the historian.—In this attempt, however, a great diversity of opinion has arisen; the origin of which it may be useful and entertaining here to trace. But, in order to render my subsequent remarks the more intelligible, I shall previously state the various dates that have

been assigned to this event by the several authors above alluded to.

PLINY places this eclipse in the fourth year of the forty-eighth Olympiad; which answers to the year 585 B. C. (*Hist. Nat.* lib. 2, cap. 12). A similar opinion has been advanced, among the ancients, by CICERO (*De Divinat.* lib. 1, §. 49), and probably by EUDEMUS (*Clement. Alex. Strom.* lib. 1, p. 354). And, among the moderns, by NEWTON (*Chron. of Anc. King. amended*), RICCIOLI (*Chron. Refor.* Vol. I. p. 228), DESVIGNOLES (*Chronol.* lib. 4, chap. 5, §. 7, &c.), and BROSSES (*Mém. de l'Acad. des Belles Lettres*, Tome 21, Mém. p. 33.)

SCALIGER, in two of his writings (*Animad. ad Euseb.* p. 89, and in Ολυμ. ἀναγραφή), has adopted the opinion of PLINY; but in another work (*De Emen. Temp. in Can. Isag.* p. 321), he fixes the date of this eclipse on the 1st of October, 583 B. C.

CALVISIUS, who was contemporary with SCALIGER, thinks that it took place in the year 607 B. C. (*Opus Chron.*)

PETAVIUS says that it happened July 9, 597 B. C. (*De Doct. Temp.* lib. 10, cap. 1): and he has been followed by HARDOUIN (*Dissert. de* LXX *hebdom. Dan.* §. 3), MARSHAM (*Chron. Canon.* p. 561), BOUHIER (*Recher. et Diss. sur Hérodot.* p. 42), and CORSINI (*Fast. Attic.* Tom. III. p. 68); together with M. LARCHER, the French translator of HERODOTUS (Tom. I. p. 335).

USHER is of opinion that it happened on the 20th of September, 601 B. C. (*Annal. Vet. et Nov. Testam.*)

BAYER has shown, from the astronomical tables then in use, that this eclipse ought to have taken place May 18th, 603 B. C. (*Com. Acad. Scient. Imp. Petrop.* Tom. III.): and he has been

supported in this opinion by the two English astronomers, COSTARD and STUKELEY (*Phil. Trans.* for 1753, pages 17 and 221).

Lastly, M. VOLNEY has attempted to show, in a recent publication (*Chronologie d'Hérodote*) that the eclipse, mentioned by the historian, could be no other than the one which happened February 3d, 626 B. C.

Thus we find a distance of no less than forty-three years between the extreme periods that have been assigned for this eclipse: an interval which, however, may be somewhat abridged; since there are other facts recorded by the same historian which enable us to reduce these limits, and yet leave the narration consistent with itself.

For, according to HERODOTUS, the two kings of Media, that immediately preceded the conquest of that country by Cyrus, were Cyaxares, who reigned forty years, and Astyages, who reigned thirty-five years: and it is admitted by all the chronologists, that Cyrus conquered Astyages in the year 560 B. C. Consequently (if the numbers given by HERODOTUS be correct) the reign of Cyaxares extended from 635 B. C. to 595 B. C. And, since the battle of the eclipse was fought in the sixth year of a war which *began after* Cyaxares had ascended the throne, it could not happen earlier than 629 B. C. nor later than 595 B. C. If therefore we can find, within this short space of thirty-four years, a solar eclipse that was central and *total* in that part of Asia bordering on the two hostile empires, where this battle was probably fought, we may justly conclude that it was the one alluded to by the historian.

I say that this eclipse must have been a *total* one, because no *annular* eclipse (and much less a *partial* one) could have

produced that degree of obscurity alluded to by HERODOTUS. The celebrated MACLAURIN, in his account of the *annular* eclipse which happened at Edinburgh, February 18th, 1737, observes (*Phil. Trans.* Vol. XL, p. 177), that "during the "appearance of the annulus, the direct light of the sun was "still *very considerable*; but the places that were shaded from "his light, appeared gloomy:"—that "day-light was *not* "*greatly obscured*; appearing only so much dimmer than "usual, as that of the sun is, when seen through a gentle "mist in a fine morning in April or May." And, as a further proof of the trifling alteration this phenomenon made, he observes, that "there was little notice taken of this eclipse by the "populace in the country: and I cannot but add, that several "gentlemen of very good credit, and not in the least short-"sighted, assure me, that about the *middle* of the annular ap-"pearance they were *not able to discover the moon upon the sun*, "when they looked without a smoked glass, or something "equivalent." In another account likewise of this eclipse, in the same volume, by Sir JOHN CLERK, Bart. it is observed that "there was *no considerable darkness*; but the ground was co-"vered with a kind of dark greenish colour." And M. LE MONNIER (who came over from France on purpose to observe the *annular* eclipse of the sun, which happened July 14th, 1748) says, "that when he looked at the sun with his naked eyes, "during the middle of the eclipse, he could observe *nothing* "*upon the sun*, but saw the sun *full*, though faint in his light." (*Phil. Trans.* Vol. XLV. p. 588).

In the account also which is given, in the *Mémoires de l'Acad. Roy. des Sciences* for 1724, of the *total* eclipse which happened on the 22d of May in that year, it is stated that, at the moment

when the *last* portion of the sun was covered by the moon, " la clarté a diminué *tout d'un coup*; de sorte qu'on a eû be- " soin de lumière pour compter à la pendule: on voyoit les " personnes au grand air, mais on ne distinguoit pas bien les " visages à quelques pas de loin." In another account, in the same volume, it is stated, that the darkness came on *dans un instant*; and that, after an interval of two minutes and sixteen seconds " le soleil *commença* à reparoitre *comme un éclair*, " qui dissipa *sur le champ* les tenébres dans lesquelles on étoit " plongé." M. DESVIGNOLES, likewise (in his *Chronologie de l'Histoire Sainte*, Vol. II. p. 253), gives an extract of a letter from M. ABAUZIT of Geneva, who, at the close of his remarks on the calculation of PETAVIUS respecting this very eclipse, observes " il ignoroit que *le moindre* raion, qui commence à " poindre, est assez fort pour dissiper les tenébres: *comme je* " *l'ai observé deux fois*." All which may serve to explain the remarkable expression of HERODOTUS, who says τὴν ἡμέρην ἐξαπίνης νύκτα γενέσθαι, " the day *suddenly* became night:" a passage which has been ignorantly censured by some of his commentators.

It appears to me, that an inattention to these singular facts has been the principal cause of the various opinions that have arisen respecting the time when this eclipse happened. For each chronologist, having a system of his own to support, has satisfied himself merely with ascertaining that a solar eclipse did take place in the year that he had assigned for it; and which eclipse he supposed might be visible in that part of the world bordering on the two hostile countries: but without taking into his account the *magnitude* of the eclipse at the place where the battle is supposed to have been fought. Now

since the territories of the two belligerent powers were probably separated by the river Halys (which was the case in the subsequent reign, although we have no authentic information that it was so at the period now under consideration), and as the battle was probably fought on the confines of these two empires, I think it will be evident from the preceding extracts, that no solar eclipse could be the one mentioned by HERODOTUS, unless it was central and *total* in some part of Asia Minor; that is, the centre of the moon's shadow, in such total eclipse, must have passed over that part of Asia Minor where the contending armies were engaging. Consequently the fact is capable of being verified or disproved by the present state of our knowledge in astronomy.

M. TH. S. BAYER is the first who seems to have fixed the attention of the public to this point, in a paper entitled *Chronologica Scythica*, inserted in the Petersburg Memoirs for the year 1728. He consulted his friend FRED. CHRIS. MAYER on this subject, who has shown, from the astronomical tables *then* in use, that neither the eclipse mentioned by PLINY, SCALIGER, CALVISIUS, PETAVIUS, or USHER, could possibly be the eclipse alluded to by HERODOTUS. For, the first two (he says) happened between the hours of sun-set and sun-rise in Asia Minor. In the third, the centre of the moon's shadow passed too near the equator, and in the last two it passed too far to the north of Asia Minor, for it to cause any remarkable obscurity there. In order, however, to set the question at rest, he calculated all the solar eclipses that could possibly be seen in Asia Minor from the year 608 B. C. to 556 B. C.; and he found that the one which took place May 18, 603 B. C. was the only one that was at all likely to be that mentioned by

HERODOTUS. According to his computation, the centre of the moon's shadow in that eclipse entered the earth's disk about N. lat. 1° 40′, and E. long. 28° from Ferrol. It proceeded, then towards the mouths of the Nile; and, traversing the Mediterranean, crossed Cyprus, Cilicia, and Cappadocia, and passed over to Trebizond.

The Rev. GEORGE COSTARD, without knowing what M. MAYER had done, has drawn nearly the same conclusions; and has likewise entered into a calculation (from Dr. HALLEY's tables) of all the eclipses which have been assigned to this event by preceding authors: which may be seen, at length, in the *Philosophical Transactions* for 1753. In a subsequent paper, in the same volume, Dr. STUKELEY has given a map containing the path of the moon's shadow in this eclipse, deduced from the calculations of a Mr. WEAVER; and which correspond nearly with Mr. COSTARD's. But Mr. COSTARD has suggested an important correction in his computation, by allowing for the moon's *acceleration*; which does not appear to have been attended to either by M. MAYER or Mr. WEAVER: and which throws the route of the moon's shadow too far to the southward to pass over any part of Asia Minor. For, on this supposition (he observes) the umbra of the moon will leave Africa near Damietta; and, after traversing the south-east corner of the Mediterranean, will enter Syria between Tripoli and Tyre: and, proceeding across Mesopotamia, between Nisibin and Mosul, will enter the Caspian sea near Ardebil. Notwithstanding this circumstance, however, the date here assigned has continued to be received as the true date of the battle of the eclipse by all succeeding chronologists; although it must be evident, even from these data, that

such eclipse could not be total any where near the place where the battle was probably fought.

But none of these calculations can have much weight at the present day, since they must have been formed from tables which the subsequent improvements in astronomy have shown to be exceedingly defective and incorrect. Even the *mean* motions of the sun and moon are not given with a sufficient degree of accuracy, either in the Rudolphine or Halleian tables, to enable us to determine, with any tolerable correctness, their true *mean* place of conjunction at so remote a period: neither can the lunar *equations*, there given, be safely depended upon. The *secular variations* also are wholly omitted: and these must have an important effect in all inquiries of this kind, since they increase in proportion to the period of time elapsed.

Under these circumstances, and in order to set this question at rest, as far as it can now be done by the aid of astronomical science, I have been induced to re-calculate the elements of the several eclipses, above alluded to, from the new *Tables Astronomiques*, lately published by the *Bureau des Longitudes* in France. In these tables, the mean motions of the sun and moon are given with the greatest exactness for the most distant periods: and, by the successive labours of MAYER, MASON, and BURG, the lunar equations are carried to an astonishing correctness; which, together with the secular variations deduced from the formulæ of M. LAPLACE, enable us to determine the true place of the sun and moon with considerable accuracy for many centuries prior to the Christian era. These calculations, at full length, together with a map containing the paths of the moon's shadow in the several eclipses there

alluded to, are sent with this paper for the inspection of the Members of the Royal Society, should they be desirous of entering more fully into the detail. The substance of those inquiries I shall now proceed to lay before them.

The eclipse, which is supposed to have been that alluded to by PLINY, happened May 28th, 585 B.C.: and the time of the ecliptic conjunction was at $2^h\ 38'\ 22''$ in the afternoon, *mean* time at Greenwich, or $2^h\ 46'\ 24''$ *apparent* time. The elements were as follow:

True longitude of the luminaries		1ˢ 29° 41′	4″
Sun's declination, north	-	20 23	17
—— semi-diameter	- -	15	45
Moon's semi-diameter	- -	16	43
—— equatorial parallax	-	61	13
—— horary motion from the sun		35	29
—— true latitude	- -	12	39
—— horary motion in latitude	-	3	30

By a projection of this eclipse, I find that the sun was centrally eclipsed on the meridian, about the middle of the Atlantic ocean, in N. lat. 33½° and W. long. 48°. The centre of the moon's shadow then proceeded to the parallel of N. lat. 40°, in W. long. 13°; where, turning to the southward, it crossed Spain, and traversed the course of the Mediterranean. By a trigonometrical calculation I have ascertained that the sun set centrally eclipsed on the borders of the Red Sea in N. lat. 28° 1′, and E. long. 35° 2′. So that at no time was this eclipse central in or near any part of Asia Minor. It happened likewise ten years *after* the death of Cyaxares, according to the received chronology.

With respect to the eclipse, which happened October 1st, 583 B. C. it is sufficient to observe that, as the ecliptic conjunction of the sun and moon did not take place till after four o'clock in the afternoon at Greenwich, it is evident that the sun must have set, centrally eclipsed, to the *westward* of any meridian line that can be drawn through any part of Asia Minor: and consequently the eclipse could not have been central in that peninsula.

CALVISIUS does not come much nearer the truth, in supposing that the eclipse mentioned by HERODOTUS is the one which occurred in 607 B. C. For in that which happened July 30th, the ecliptic conjunction took place at $8^h\ 26'\ 18''$ in the morning, *mean* time at Greenwich, or $8^h\ 35'\ 45''$ *apparent* time: and the elements were as follow:

True longitude of the luminaries	3^s	29°	$6'\ 54''$
Sun's declination, north -		20°	$38'\ 39''$
—— semi-diameter - -			$15'\ 54''$
Moon's semi-diameter - -			$15'\ 10''$
—— equatorial parallax -			$54'\ 33''$
—— horary motion from the sun			$27'\ 41''$
—— true latitude, south - -			$2'\ 17''$
—— horary motion in latitude -			$2'\ 46''$

By a trigonometrical calculation, I find that the sun rose centrally eclipsed off the coast of Sierra Leona in N. lat. $8^\circ\ 13'$ and W. long. $12^\circ\ 33'$. The moon's umbra then crossed the continent of Africa between the 10th and 20th degrees of north latitude: and the sun became centrally eclipsed on the meridian in Arabia Felix, in N. lat. $18\frac{1}{2}^\circ$ and E. long. $3^\circ\ 24'$. It is evident, therefore, that this eclipse (independent of its

being *annular*) was not central in any part of Asia Minor. The other eclipse in this year, which took place February 2d, happened when it was near midnight in Asia Minor.

The eclipse mentioned by PETAVIUS took place July 9th, 597 B. C. The ecliptic conjunction happened at $4^h\ 29'\ 25''$ in the morning, *mean* time at Greenwich, or $4^h\ 29'\ 58''$ *apparent* time: and the elements were as follow:

True longitude of the luminaries	3s	9° 16′	32″
Sun's declination, north - -		23 28	18
—— semi-diameter - -		15	49
Moon's semi-diameter - -		14	50
—— equatorial parallax -		54	23
—— horary motion from the sun		27	32
—— true latitude - -		41	59
—— horary motion in latitude		— 2	44

By a trigonometrical calculation, I find that the sun rose centrally eclipsed to the inhabitants of Holland in N. lat. 51° 45′ and E. long. 5° 39′. The moon's umbra then proceeded across Denmark, Finland, and the northern provinces of Russia: and the sun became centrally eclipsed on the meridian in N. lat. 74½° and E. long. 113° 35′. This eclipse, therefore, could not possibly be the one mentioned by HERODOTUS. And yet his translator, M. LARCHER, without taking the slightest pains to verify the fact, or even to ascertain its probability, has adopted it as the most likely one, "parcequ'elle s'accorde mieux avec la chronologie que toutes les autres:" an opinion as unfounded, as the circumstance to which it relates; and an assumption which puts the visionary speculations of the antiquarian in competition with the immutable laws of nature. It

is scarcely necessary to add, that this eclipse likewise was *annular*.

In the eclipse alluded to by USHER, September 20th, 601 B. C. the ecliptic conjunction took place at $7^h\ 23'\ 18''$ in the morning, *mean* time at Greenwich, or $7^h\ 31'\ 35''$ *apparent* time: and the elements were as follow:

True longitude of the luminaries	$5^s\ 20^\circ\ 46'\ 50''$
Sun's declination, north - -	$3\ 42\ 27$
—— semi-diameter - -	$16\ 8$
Moon's semi-diameter - -	$16\ 48$
—— equatorial parallax -	$61\ 14$
—— horary motion from the sun	$35\ 24$
—— true latitude - -	$52\ 1$
—— horary motion in latitude	$-3\ 27$

From a projection of this eclipse, it will be seen that the centre of the moon's shadow entered the earth's disk very near the north pole; and that the sun became centrally eclipsed on the meridian in N. lat. $73\frac{1}{4}^\circ$ and in E. long. $72^\circ\ 10'$. The umbra then passed over Siberia and the eastern parts of the Chinese empire: and consequently this eclipse was not central in any part of Asia Minor.

The eclipse first suggested by BAYER, and hitherto generally received as the true one, happened May 18th, 603 B. C. The ecliptic conjunction took place at $7^h\ 12'\ 13''$ in the morning, *mean* time at Greenwich, or $7^h\ 19'\ 36''$ *apparent* time: and the elements were as follow:

True longitude of the luminaries	$1^s\ 19^\circ\ 15'\ 44''$
Sun's declination, north -	$17\ 48\ 24$
—— semi-diameter - -	$15\ 46$

Moon's semi-diameter - -	16′ 43″
——— equatorial parallax -	61 16
——— horary motion from the sun	35 32
——— true latitude - -	17 15
——— horary motion in latitude -	3 30

By a trigonometrical calculation, I find that the sun rose centrally eclipsed in S. lat. 5° 9′ and E. long. 0° 46′. The moon's umbra then passed over the continent of Africa in a north-easterly direction; and, crossing the Red Sea, entered Arabia near Mecca, continuing its course over the provinces of Kerman and Segistan in Persia. The sun afterwards became centrally eclipsed on the meridian in N. lat. 35¼° and E. long. 68°. Consequently this eclipse could not be central in any part of Asia Minor: and yet it has generally been considered, of late years, as the only one that could be reconciled to the fact.

Lastly, I shall notice the eclipse proposed by M. VOLNEY, which happened February 3d, 626 B. C. The ecliptic conjunction took place at $4^h\ 19'\ 27''$ in the morning, *mean* time at Greenwich, or $4^h\ 0'\ 35''$ *apparent* time: and the elements were as follow:

True longitude of the luminaries 10°	7° 47′ 47″
Sun's declination, south -	18 35 50
——— semi-diameter - -	16 7
Moon's semi-diameter - -	15 16
——— equatorial parallax -	55 56
——— horary motion from the sun	29 13

Moon's true latitude - -	44 28
——— horary motion in latitude	— 2 53

By a trigonometrical calculation, I have ascertained that the sun rose centrally eclipsed to the inhabitants of Great Buccharia in N. lat. 40° 17′, and E. long. 61° 35′: and the moon's umbra then proceeded in a south-easterly direction across Thibet and China. Consequently this eclipse (which, moreover, was an *annular* one) could not possibly be central in any part of Asia lying to the west of the Caspian Sea: and M. VOLNEY ought to have taken some steps towards ascertaining this fact, before he ventured to set up his own opinion in opposition to all preceding chronologists.

I have thus shown, from the most correct evidence which the present state of astronomical science affords, that not one of the eclipses, mentioned by either of the authors above alluded to, could possibly be that which is recorded in so singular a manner by HERODOTUS. In order, however, that I might not leave the subject in the same degree of doubt in which I found it, I have taken the pains to calculate all the solar eclipses that were likely to have been visible in Asia Minor, from the year 650 B. C. to 580 B. C.: but, out of this period of seventy years, I have found only one that was central in, or *near*, any part of that peninsula.

The eclipse here alluded to, happened September 30th, 610 B. C. The ecliptic conjunction took place at $8^h\ 19'\ 51''$ in the morning, *mean* time at Greenwich, or $8^h\ 21'\ 41''$ *apparent* time: and the elements were as follow:

True longitude of the luminaries	5s	29°	39′ 40″
Sun's declination, north	-	-	0 8
—— semi-diameter	-	-	16 10
Moon's semi-diameter	-	-	16 36
—— equatorial parallax	-		60 50
—— horary motion from the sun			34 53
—— true latitude	-	-	29 57
—— horary motion in latitude		-	3 26

Since the sun's declination in this eclipse was only eight seconds, it may safely be neglected in the calculation; and it may then be found very easily by plane trigonometry that the sun rose centrally eclipsed in N. lat. 47° 34′, and W. long. 11° 55′; that it was centrally eclipsed on the meridian in N. lat. 31° 6′, and E. long. 59° 33′; and set centrally eclipsed in N. lat. 11° 13′, and E. long. 122° 36′. The centre of the moon's shadow crossed the parallel of N. lat. 42° in E. long. 84° 45′; and the parallel of N. lat. 36° in E. long. 50°: and consequently passed nearly in a straight line over the north-eastern part of Asia Minor, through Armenia and Persia, where the sun became centrally eclipsed on the meridian, as above-mentioned. This eclipse, therefore, was central and total to part of Asia Minor, Armenia, and Media: and the path of the moon's umbra lay in the very track where the two hostile armies probably met. For it passed over the very mouth of the Halys, just at the point where Crœsus, the immediate successor of Alyattes, crossed that river in order to attack the Median empire.

It would appear from the order of events belonging to the reign of Cyaxares, as related by Herodotus, that the battle of

the eclipse happened *prior* to the invasion of the Scythians, who kept possession of his kingdom twenty-eight years; and that, after the expulsion of those barbarians, he besieged and took the city of Nineveh, and thereby put an end to the Assyrian empire. This, however, will not accord with the date here assigned: neither indeed will it suit any of the systems above alluded to; except it be that of M. VOLNEY, which may lay claim to some ingenuity. But his system is too much at variance with the astronomical fact to be entitled to any credit.

It has been remarked by Dr. HALLEY (*Phil. Trans.* Vol. XXIX. p. 245), that "though twenty-eight eclipses of the "sun happen in eighteen years, and eight pass through the "parallel of London, yet since March 20th, 1140, no *total* "eclipse has been seen in that metropolis." Indeed, so rare is this phenomenon in any particular country, that its occurrence, when well authenticated, may be considered as an era which is less liable to mistakes or confusion, than any other event recorded in history. All attempts at imposition or deceit are easily detected by our knowledge of astronomy: and the unintentional errors of the historian are soon rectified and adjusted. On this account, and as the fact of the eclipse is so confidently related by HERODOTUS (indeed, its singular coincidence with the battle will ever render it memorable in history), I would place the termination of the war between Alyattes and Cyaxares, in the year 610 B. C.: and, if the other events of that period, as related by the historian, cannot be reconciled to this date, I should attribute the confusion to the want of authentic documents and information at the time that the history was written.

I have before observed, that all these calculations have been made from the *Tables Astronomiques*, lately published in France: which tables have since been adapted to the meridian of Greenwich, and to astronomical time, by Mr. VINCE, and inserted by him in the third volume of his System of Astronomy.* In these tables are given the *secular variations* in the moon's mean longitude, mean anomaly, and mean distance from her node, as deduced from the formulæ of M. LAPLACE. It is with much deference that I presume to question the accuracy of the results, obtained by means of those formulæ; but, as the present subject is in a great measure connected with that inquiry, I shall briefly state my reasons for offering a doubt upon that point.

It is well known that AGATHOCLES, king of Syracuse (when besieged in that city by HAMILCAR, the Carthaginian general), undertook the bold design of invading Africa, and thereby moving the seat of war from Sicily. He accordingly embarked a numerous army, and set sail for the continent. The *day after* he left Syracuse, the fleet was terrified at an eclipse of the sun; which was so great, that, in the words of DIODORUS

* It is to be regretted, that Mr. VINCE did not adapt his tables to the English system of *chronology* likewise. For the years before Christ, according to the *English* mode of computation, exceed by unity the corresponding years given by the *French* chronologists: since they make the year of Christ equal to 0, whereas the English reckon it as 1 B. C.—The French also assume the year 1582 as the date of the Reformation of the Calendar; whereas, in England, that event did not take place till the year 1752.

Without a proper attention to these circumstances, we may be led into an error of one whole year, in the calculation of the places of the heavenly bodies for any period prior to the Christian era; and into an error of ten or eleven days in our calculations for that space of time which is included between October 5th, 1582, and September 14th, 1752.

SICULUS, lib. 20, ὁλοσχερῶς φανῆναι νύκτα, θεωρουμένων των ἀστέρων πανταχῆ, "it seemed *exactly* like night, the stars *every where* "appearing." This eclipse was therefore evidently *total* in the place where it was seen by the fleet of AGATHOCLES. It happened on August 15th, 310 B. C. The ecliptic conjunction took place at $8^h\ 10'\ 23''$ in the morning, *mean* time at Greenwich, or $8^h\ 9'\ 6''$ *apparent* time: and the elements were as follow:

True longitude of the luminaries	$4^s\ 16^\circ\ 41'\ 32''$
Sun's declination, north -	$16\ 2\ 38$
—— semi-diameter - -	$15\ 57$
Moon's semi-diameter - -	$16\ 39$
—— equatorial parallax -	$61\ 0$
—— horary motion from the sun	$35\ 9$
—— true latitude - -	$14\ 42$
—— horary motion in latitude -	$3\ 28$

From these elements I have found, by a trigonometrical calculation, that the sun rose centrally eclipsed to the inhabitants of the western coast of Africa, in N. lat. $24^\circ\ 57'$ and W. long. $14^\circ\ 9'$. The centre of the moon's shadow then, crossing the desert, proceeded towards the Mediterranean, near to, but rather to the southward of, Tripoli; and crossed the parallel of N. lat. 33° in E. long. $20^\circ\ 44'$. But in no part of its course did it advance more northerly than N. lat. $33^\circ\ 55'\ 36''$, which I find by a trigonometrical calculation to be its maximum of latitude, and the parallel of which it reached in E. long. $35^\circ\ 21'\ 8''$. It then turned to the south; and the sun became centrally eclipsed on the meridian in N. lat. $30\frac{1}{4}^\circ$ and E. long. $59^\circ\ 45'$.

Let us now compare this result, with the fact as related by DIODORUS. It is stated by this author, that AGATHOCLES was six days on his passage, from Syracuse to the coast of Africa; although he used the utmost expedition, being, in fact, closely pursued by the Carthaginian fleet. The place where he landed was called Λατομίας, the *Quarries;* whence he proceeded to the neighbouring cities of Μεγάλην πόλιν, *Megalapolis*, or the *Great City*, and Λευκὸν Τύνητα, *White Tunis*. The position of these cities is not handed down to us; all that we know is, that the latter place (which must not be confounded with the present Tunis) was two thousand stadia, or two hundred and twenty-nine English miles, distant from Carthage. AGATHOCLES, therefore, probably landed near the Syrtis Minor, or Gulph of Cabes, about three hundred miles in a direct course from Syracuse: whence we may reasonably conclude that he performed one-sixth of his passage, or about fifty miles, in the space of one day; which, I am aware, is not so much as the mean rate that has been attributed to the ships of the ancients (see HERODOTUS, lib. 4, §. 86). Syracuse lies in N. lat. 37° 3′ and E. long. 15° 14′; and, consequently, on the day after the sailing of AGATHOCLES from that port (being the day on which the eclipse took place), the fleet would be in about N. lat. 36¼°: at all events, it could not (from the direction of its course) be much farther south than this point, which is all that is required in the present instance; and a few miles, either way, not being of any material consequence. It follows therefore, that in the meridian of Syracuse, the northern part of the moon's umbra ought to extend as far north as that parallel of latitude. But, from the calculations above adduced, it

will be found that the centre of the moon's shadow, on that meridian, had only reached the parallel of about N. lat. $32\frac{1}{2}°$: and as the semi-diameter of the umbra was not more than forty-seven and a quarter English statute miles, or about two-thirds of a degree, the eclipse could not *there* be total to the northward of N. lat. $33\frac{1}{4}°$. Now, since the place where AGATHOCLES landed in Africa, was probably not situated below the parallel of N. lat. $34°$, it is evident that he did not, in *any part* of his course (and much less, at the *commencement* of it), come within a considerable distance of the moon's umbra.

I much doubt whether, according to our present computation, this eclipse was total even at Tripoli: and, although it was unquestionably of considerable magnitude, both there and as far north as Syracuse itself, yet (for the reasons already given in this paper) I do not think that, at any intermediate place between these two cities, it could be so great as to produce that degree of obscurity, which is recorded by DIODORUS and confirmed by JUSTIN. In order that the phenomenon should accord with the fact, as related by these historians, the centre of the moon's shadow ought to pass over, or very near to, Malta: that is, the latitude of the moon ought to be, at least, three degrees greater than our present tables make it.

Since the latitude of the moon depends on her true distance from the node, these observations (if correct) will show the necessity of some alteration in the table of the secular variation of the moon's mean distance from her node, which (agreeably to the rule given by M. LAPLACE) is deduced immediately from the secular variation of her mean longitude. These remarks, however, are thrown out merely as hints to those who

are more conversant with, and better informed on, the subject: and I regret that I have not more time to pursue the inquiry farther.

Such an alteration, as is here suggested, would somewhat vary the position of the route of the moon's umbra, in all the eclipses which have been the subject of this paper; but, in none of them would it alter the conclusions which have been drawn from them, except perhaps in the one (Sept. 30th, 610 B. C.) which I have supposed to be that mentioned by HERODOTUS. In this particular case, the path of the moon's umbra might, by such a correction, be thrown so much farther north as to prevent the eclipse being total in any part of Asia Minor. But still it would remain the only one that can be at all adapted to the account given by HERODOTUS; since there is no other that could possibly be central in, or *near*, any part of Asia Minor from the year 650 B. C. to 580 B. C.: a period which far exceeds the probable limits of time wherein this singular phenomenon must have taken place, so as to be reconcileable to any received system of chronology.

F. B.

November, 1810.

XIII. *An Account of the great Derbyshire Denudation. By Mr.* J. Farey, Sen. *In a Letter to the Right Hon. Sir* Joseph Banks, *Bart. K. B. P. R. S.*

Read March 21, 1811.

Sir,

I had but recently entered on the survey of Derbyshire and its environs, which under your kind patronage I was induced to commence in the autumn of 1807, and had only cursorily examined the strata, in my way from Charnwood Forest and Breedon in Leicestershire, in order to meet you at Overton Hall, before I perceived clearly, that those principles which contemplate the terrestrial strata as terminating or *ending* in one direction (simple and important as they are), which I had learned under Mr. William Smith in 1801, and which he has so successfully applied in the filling up of his maps of the strata in the south-east and east, and some of the middle parts of England, would fail me, in their application to the strata of Derbyshire, without taking into consideration along with them, not only the denudation, or local stripping off, of patches of strata, some of immense extent and thickness, and even more considerable than those which I had discovered to be missing*

* And such as Dr. William Richardson had found to have been removed, in several places, from off the basaltic area in the counties of Derry and Antrim in Ireland, and has named *abruptions*, in his very admirable paper on this district, in the Philosophical Transactions for 1808.

from off the Wealds of Kent, Sussex, and Surry, and had explained to you, by a rough section across this great southern denudation in 1806, and such as the valley of Ashover then appeared to present, a more perfect instance of, around us: but that previously to such denudations of the Derbyshire strata, immense dislocations or vertical derangements of very large piles of strata, separated by the fissures, called *faults* by the miners, needed also to be taken into account, for explaining the appearances of the strata and surface of the district, which I was then about to explore: *faults*, exceeding immensely in their extent and quantity of *lift* on one side (or sink on the other) any which had occurred to Mr. SMITH, in the tracing of the south-eastern strata of England, where no faults had been discovered, so considerable as to cut off entirely the connection of the strata, or in other words, to bring strata in contact on the surface, whose places in the series were too distant to be known, and readily traced in their order, in the neighbourhood. And in consequence, I judged it necessary, on my return to town, when the winter arrived, to set about the consideration of stratified masses, broken and dislocated, and then cut or denudated in all the variety of cases and degrees of each, the results of which investigation, will appear in my Report to the Board of Agriculture on Derbyshire, the first volume of which is now in the press.

With ideas thus extended, I found, on resuming my Survey in the spring of 1808, that some conclusions that I had formed, and had unfortunately committed to paper, in a sketch of a section across the county, were erroneous, and that immense *faults* occurred, in places where their existence had not been proved by miners, or generally understood, which combined

with the denudations, that were so apparent in my first journey across the county in the preceding autumn, offered, as I proceeded afterwards in filling up my map, a considerably different explanation of the structure of the county, or section of its strata, from that which I had previously made, and permitted some persons to copy. The first volume of my Report to the Board of Agriculture, abovementioned, has compressed into it, all the most essential particulars of my Survey, which manuscript you did me the honour to examine, and to recommend its adoption to the Board; but as the plan of that Report did not admit of taking an extended or connected view of the great faults or dislocations of the district, I have troubled you with this Letter, in order to describe them: previous to which it may be right just to recall to your recollection, a few particulars respecting the British stratification. It is now well known to great numbers of observers, that the thick clay and other strata, on which the metropolis is situated, extend eastward through Essex, Suffolk, and Norfolk to the eastern coast, and in all their extent cover the chalk strata: that these again (the chalk) extend from the Isle of Wight to Flamborough Head, and cover other known strata, which have their regular *basset-edges*, or appearances at the surface, in continuity, to the westward of the limits of the chalk, and of each other; and thus it has been imagined by many, that the whole surface of England could be referred to, or explained by, an uninterrupted series of basset-edges of strata, dipping to the SE. and ranging in continuity from SW. to NE. in certain undulating lines, conformable to the surface, from one sea to the other, just as a certain number at the upper part of the series have been shown to do, by Mr. SMITH'S manuscript maps. But, after

passing the edges of the *lias* limestones and clay strata, in our progress to the westward, from any of the south-eastern and eastern parts of England, we find on the surface marks of an immense stratum of red earth or marle, which basseting from under the lias clay and sand, seems once to have extended over all the remainder of the British islands, without being now any where covered by patches of upper strata,* much beyond the continuous edge of the lias strata, abovementioned. Instead, however, of seeing the middle and all the western and northern parts of Britain covered by the same red strata, we find now, in this space, numerous local and many very large tracts of strata, surrounded by vertical and connected *faults*, and greatly lifted and tilted; from the surface of which lifted tracts, the upper red earth, and vast and very unequal thicknesses of strata, that lay in regular succession below this red earth, have been denudated, "abrupted," or carried off, leaving thus, a great variety of what have been called coal-fields, or *mineral-basins*,† in which limited tracts, great and most important series of strata, are to be seen basseting (owing to the local denudations), of which the basset-edges, or continued endings, can no where be traced in these islands, as far as I can learn. Large tracts of the intervening spaces, between these denudated mineral basins, are still occupied by the red marle, containing local strata of gypsum, rock-salt, sand, micaceous grit-stone, &c. &c. in its substance, or exposed by denudation; and in others, local strata, or

* Gravels, peat, &c. not being included in this term.

† Of which a fine instance is described by Mr. Edward Martin, in the Philosophical Transactions for 1808, and of which the Forest of Dean presents a smaller, but similar instance.

nodules of great extent, or rather, perhaps, rudely crystallized masses of slate, green-stone, sienite, basalt, &c. &c. forming hills or mountains (often intersected by mineral veins) from the tops of which masses, the red marle has in most instances been denudated. It remains a task of great difficulty, yet to be accomplished, to ascertain the lower part of the British series of strata, thus only exposed to view, in local and unconnected tracts, or basins, which are in part often concealed by gravel (frequently so, near their borders), and towards which investigation, little has yet been done. It seems to me, that there are three distinct series of coal-measures, if not more, separated by thick strata of red earths, or marles, not easily distinguished from the upper one above the coal series, or that which underlays the lias strata, as abovementioned, and by thick strata of limestones; each of which red earths, probably, produce anomalous and local strata, or crystallized mountain masses, in different places, where they form the surface, and the fact of such containing no organic remains, may not have arisen from their having been formed before organized beings existed, as those contend who call them *primitive* rocks, but because the circumstances proper to crystallization, were unfitted to the propagation and life of either animals, or vegetables; and may it not be doubted, whether crystallized masses, great or small, are ever the seats of reliquia?

The northern part of Derbyshire, and the adjoining parts of the surrounding counties, present a denudated tract, and partake of this uncertainty, as to what place in the lower part of the British series of strata, its strata should be referred: from many circumstances, I am inclined to consider the coal-

field of Derbyshire, Nottinghamshire, and Yorkshire, underlaying the yellow-lime rock, as lower in the series than any others of the coal-measures alluded to above, and that the fourth limestone rock, which extends from Castleton in Derbyshire, southward to Weaver Hill, near Wooton and Ramsor in Staffordshire, is the very lowest which is known in Britain, and which may account for the circumstance, that the mineral veins and the strata in which they occur in Derbyshire, present some phenomena, which are said to occur no where else.

I shall proceed now to describe the circumstances, under which this great elevation and denudation of part of the Derbyshire strata seems to have happened, which is, by a series of three or four separately lifted tracts, one within the other, as represented in the small sketch map annexed. The outer or least lifted of these tracts is bounded on the south by a fault, that I have distinguished by a full line, where ascertained, and by slight dots where only inferred, and denominated it the great Derbyshire fault, which is perfectly defined from near Nottingham across Derbyshire, to the north side of Stone in Staffordshire (except in a few places where gravel covers it), by having red marle, lying nearly horizontal, on all its south side, and different strata on its north side, as will be mentioned further on: the eastern fault or side of this first raised tract is not visible within the limits of my Survey, like the southern, on account of the vast accumulation of quartz gravel in Sherwood Forest, and the peaty alluvia north of it: but it seems probable to me, that its range is from about the town of Nottingham, east of Mansfield, east of Worksop near Bawtry, west of Thorne in Yorkshire, and how much further

north this fault proceeds, before it turns to the west, I am unable to state from my own observations; but from the correspondence of my friend WILLIAM SMITHSON, Esq. of Heath Hall near Wakefield, a very able observer, I conclude, that the boundary fault on the north of the outer lifted tract, ranging not far from the lower part of the course of the Wharf river, suddenly cuts off, or terminates the great Derbyshire and Yorkshire coal-field to the north, and continues S. of Otley and Keighley * near Colne in Lancashire and Clitheroe, bounding still the coal-field of Lancashire to the north. I am not sufficiently acquainted with the Lancashire strata to hazard a conjecture, as to where this fault turns (or branches perhaps) towards the southward again; but on the west it probably passes not far from Manchester, Stockport in Cheshire, Macclesfield, Congleton, Church-Lawton Salt-works, and joins the great Derbyshire fault, or southern boundary of this very large raised tract, somewhere to the NW. of Stone in Staffordshire, as I judge, from the information which I have received, of the red marle occupying the surface withoutside this raised tract to the westward, in Cheshire and Staffordshire,

* It seems probable, from the accounts which I have received from Mr. SMITHSON, of the many small coal-basins, or swilleys, as they are called, which occur in the space between Keighley, Hawes, and Richmond, viz. on the N. side of Keighley; on Fountain Fell in Craven; Thorpe Fell near Burnsall, Threshfield near Linton, and Anter-Heights near Kettlewell, on the Wharf river; Netherdale Forest near Middlesmoor, on the Nidd river; Slapestones near Hawes, West-Scrafton SW. Leyburn NW. and Braithwait-Bank near to Middleham on the Yore river; Hudswell Moor SW. of Richmond on the Swale river, &c. that all these belong to the lower or calcareous part of the Newcastle coal series, as exhibited in Mr. WESTGARTH FORSTER's "Treatise on a Section of Strata," lately published, wherein near 4100 feet thick of strata are described in order.

as well as south of it in Staffordshire, Derbyshire, and Nottinghamshire, as abovementioned, and on the east of it from Nottingham to Thorne in Yorkshire, and perhaps further northward.

This border, or plain of red marle, has the tract within it so raised, that the yellow, or magnesian lime rock, probably abuts against the marle at the surface of the strata on the east side, under the gravel, &c. from near Nottingham to near Wetherby in Yorkshire. From Lenton E. of Nottingham, to Allestry N. of Derby, the upper parts of the coal-measures in the first raised tract, abut on the red marle: here another great fault, called the zig-zag fault, intersects the boundary fault; from Allestry to the SE. corner of the Weaver Hills near Wooton in Staffordshire, the second inner tract, with a vastly greater rise than the first, abuts on this southern fault, so as to bring the great limestone-shale (which underlays all the coal-measures) against the red marle on the surface; at this SE. corner of the Weaver Hills, another great fault (called the great limestone fault) intersects the southern boundary (or great Derbyshire) fault of the raised tract; and from this place to the SW. corner of the Weaver Hills near Ramsor, a third inner tract, with four hundred yards or more of perpendicular rise, in addition to the last, occasions the fourth, or lowest limestone rock, to abut against, and even make a high hill above the red marle at the foot of it, on the other side of the great Derbyshire fault; which here occasions a sudden derangement of the strata (and a corresponding denudation of the large tract of country to the northward has taken place), far exceeding any thing which has hitherto been mentioned by authors, or conceived probably by any one.

At the SW. corner of the Weaver Hills abovementioned, the great limestone fault again leaves the south boundary, or great Derbyshire fault, and proceeds northward, after which a corner of the second interior raised tract again presents itself, and the limestone-shale again abuts on the marle, as we pursue the great Derbyshire fault to the westward, owing to the rise being less here by four or five hundred yards, than it was in the third interior tract; but as we proceed southwestward, owing to the dip of the measures on the N. side of the great Derbyshire fault towards the west, the first grit, the first coal-shale, and the second grit rock successively abut against the marle, before the gravel covering commences, east and south of Cheadle, which prevented my tracing this fault any further, within the limits of my Survey.

It seems probable, however, that somewhere SW. of Cheadle in Staffordshire, a branch sets off from the great Derbyshire fault, or southern boundary of the lifted tracts, and proceeds northward, near to Endon and Bosley in Cheshire; the triangular tract beyond which, to the westward, shewn in the map, forming the pottery coal-field, is much less raised, perhaps, than any of the other tracts which have been here mentioned.

If we return to Allestry N. of Derby abovementioned, and trace the zig-zag fault, through Little Eaton, West Hallam, and Ilkeston in Derbyshire, Awsworth, Greasley, Annesley, Kirkby, and Dirty-Hucknal in Nottinghamshire, Alt-Hucknal, Bolsover, Clown, and Barlborough in Derbyshire, Harthill, South-Anston, North-Anston, Dinnington, &c. in Yorkshire, we shall find coal-measures on both sides of it, through its whole length, except in two or three instances, where the

yellow lime strata at the top of these coal-measures abut against it for short distances, and between Allestry and Little Eaton, where the limestone-shale below these coal-measures abuts on its W.; but the rise is very considerable and unequal on the N. and W. sides, through its whole length, compared with the other sides, as I have particularly shewn in my Report to the Board, and pointed out the great difficulties which this zig-zag fault has presented, to the right understanding the entire of the great Derbyshire and Yorkshire coal-field, by the many very intelligent and able colliers who are found in it.

The first, or outer raised tract, thus bounded by faults (except, perhaps on the N. where my Survey has not extended), shews no very rapid dips or inclinations of the strata, except in very limited spots, and presents on the surface either the yellow lime rock, or the parts of the coal-measures not very far beneath that rock, compared with the whole thickness of these coal-measures.

The great limestone fault (which has been mentioned above) commences in the town of Cromford in Derbyshire, in the first or upper limestone rock, and proceeds through Middleton, Wirksworth, Hopton, Carsington, Ballidon, Parwich, Newton-Grange, and Thorpe in Derbyshire, Ilam, Blore, and Thornwood in Staffordshire, and joins the great Derbyshire fault near Wooton (as abovementioned), with which it coincides along the S. end of the Weaver Hills to near Ramsor, where it again leaves this fault and proceeds near Caldon, Water-Houses, Water-fall, Grindon, Wetton Mill, S. end of Ecton Hill, near Gateham and Narrowdale in Staffordshire, Wolfscote, Beresford, Hartington, Ludwell,

Pilsbury, Crowdycote, Dowall, Booth, Thirkelow, Edge-end, Buxton Baths, Black-edge, Dove-hole, crosses to the W. side, and again to the E. side of the *Grand Ridge** of the island, passes near Sparrow-Pit, Perry-foot, Odin-Mine, Lane-head, Castleton Town, Pindale, Edingtree, Bradwell, Hazlebadge, Quarters-house, and Windmill-houses, and terminates in the first lime rock between Wardlow-Mires and Litton in Derbyshire. If now a line be traced on the same first lime rock, through Wardlow, W. of Little Longsdon, W. of Ashford, through Sheldon, Callenge Low, Middleton by Yolgrave, S. of Gratton, Elton, Winster, Wensley, and Snitterton, W. of Matlock Church, Starkholmes, and Willersley Castle to Cromford Town, shewn by very fine dots in the map, this line on the first limestone, may be considered as a kind of hinge, or joint, on which the second inner raised tract, and the third inner raised tract have turned a little, and altered their inclinations with respect to each other and the surrounding tracts (without any vertical derangement at this hinge), so that the great limestone fault above described, from Middleton by Wirksworth, round to the westward through Staffordshire, as above, to Quarters-house near great Hucklow (with the exception of the short distance between Wooton and Ramsor, and some other trifling ones) has the limestone-shale (or the shale-limestone, &c. belonging to it) on its outside on the surface of the second inner raised tract, for more than fifty miles; but on its other side, owing to the great tilt or rise of the western side of the third inner raised tract, if we begin in Cromford, and pursue the course of the great limestone fault, up Bonsal-Dale, we have at first the first lime on its right or N. side (as

* See that article in Dr. REES's Cyclopædia lately published.

well as on its S. side), then the first toadstone, next the second limestone, then the second toadstone, and after its turning to pass through Middleton by Wirksworth, the third limestone also abuts against it, and continues so to do, till the third toadstone appears against it at Hopton, and then the fourth limestone, or lowest known rock of the district, abuts against it all the way round, through Staffordshire to Castleton (with the exception of the hummocks of third limestone at Buxton and at Barmoor in Peak Forest, and a few sunk gulfs of shale) through a length of more than forty-five miles. At the SE. end of Castleton Town, the third toadstone abuts again on the right or S. side of this great limestone fault, and from thence to the SW. side of the Windmill-Houses, the third limestone abuts against it, then the second toadstone, the second limestone, the first toadstone, and at length the first limestone, before the fault terminates or becomes too inconsiderable to be readily traced, owing to its no longer deranging the order of the strata on the surface, but has the first limestone on both its sides; and thus it happens, that the third inner raised tract, or mineral-field, consisting of the four limestone rocks and three interposed toadstones, and containing about 105,000 acres, has about 51,500 acres of these on its eastern side, occupied by the three upper limestones and the toadstones, and the remaining 53,500 acres in Derbyshire and Staffordshire is occupied by the fourth, or lowest limestone rock, in which only a few mineral veins occur among its numerous open fissures and caverns.

In the sketch map which accompanies this, I have shewn a smaller, or fourth inner raised tract, on which Bakewell is situated, in great part surrounded by a fault, which I have

thence denominated the great Bakewell fault: I have selected this tract, among other small local raised ones, on account of its approaching in shape and position to the others which surround it, and because it explains several curious appearances of the strata in these parts, which have been, and are still likely to be greatly misrepresented.

The fault to which I allude, may be said to commence in the limestone-shale on the E. side of Beeley, to pass on the S. side of the village, across the Derwent and on the S. of Haddon-Hall, continuing across the pastures to the Lathkil river about a quarter of a mile above Alport, then turns S. past the W. end of Alport to the upper mill, thence WSW. for about one mile, where this fault again turns to the N. and crosses the Bradford river, and proceeding across the Meadow-place Liberty, crosses Robinstye Mine and the Lathkil river above Over-Haddon mill; bears then a little to the east of the north, and crosses the new Bakewell and Buxton road at the rise of the hill, passes the N. end of Bird's-head Mine, crosses the Wye river about a quarter of a mile above Bakewell Cotton Mill, proceeds near to Rowdale, turns E. towards Nether Burchill, then NE. following nearly the course of the brook, it continues the same direction until about half a mile E. of Hassop, where it turns to the E. crosses the Derwent again half a mile above Baslow, and terminates in the limestone-shale in Barbrook Dale, as it began. The western side of this fourth inner tract being most raised (similar to the third tract) occasions the great elevation of the shale, and its freestone in the hills E. of Bakewell and NE. of Haddon-Hall; the sudden appearance of the limestone, on which Haddon-Hall stands, and in the quarry SSW. of it, on the W. of the

road, and thence across the pastures to the Lathkil river; the sudden elevation of the limestone knowls SW. of Yolgrave, near the shale; and by this same lift, it happens, that the vale of the Bradford suddenly cuts through the first lime rock, as soon as it has crossed this fault, and shews the first toadstone to a considerable height up each side of the valley, but which declines with the dip of the measures in this tract, until the Bradford again gets upon the first toadstone, and then on the first limestone. In like manner, the greater rise of the measures at this fault, on the SW. of Over-Haddon village occasions the valley of the Lathkil river, which till then had been excavated in the first lime rock, to enter abruptly so deep into the first toadstone, as to lay bare a patch of the second limestone under it in the river, both of which however descend again below the bed of the river, before we get down to the crossing of the Ashburne turnpike road.

This fault also occasions the sudden appearance of shale-limestone on the surface NW. of it, opposite to first limestone on the other side in Bakewell Fields, and of the first toadstone on the NW. of Bakewell Cotton-Mill, almost excavated through by the vale of the Wye river, where it abuts against shale or shale-limestone at the northern end of this noted patch of toadstone, the situation and circumstances of which, when compared with those of the other two patches, at the edge of this same raised tract, as above, will be divested of much of that singularity which has been ascribed to it; for we see, that each of the three rivers, which pass on to this fourth inner raised tract, have their excavations cut through the first limestone, so as to expose the first toadstone for some distance, until the more rapid descents of the measures than

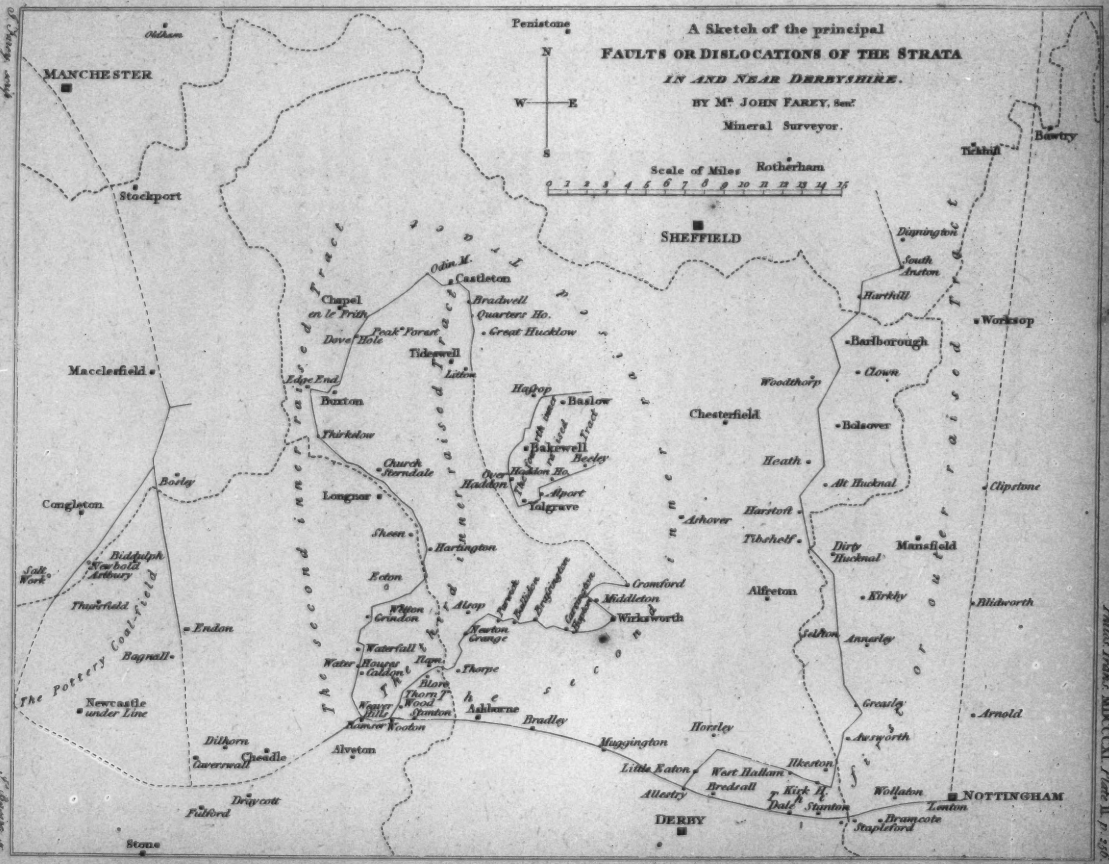
of the vales, occasion them again to dip and disappear in the bottoms of each of these vales.

I am, Sir,

your obliged and very humble servant,

J. FAREY, SEN.
Mineral Surveyor.

Upper Crown-street, Westminster,
January 31, 1811.



Phil. Trans. MDCCXI. Plate II. p. 276.

XIV. *An Account of an Appendix to the small Intestines of Birds.* By James Macartney, *Esq. F. R. S.*

Read March 21, 1811.

Every author who has written upon the incubation of the egg, except Leveille', has admitted the existence of a direct communication between the yolk bag and the small intestine of the chick, to which the name has been sometimes given of Ductus Vitello-intestinalis. It was likewise known to Stenon, Needham, and Maître Jan, that this duct remains in the form of a small cœcum during life; but I do not find that any anatomist was acquainted with the great size that this part possesses in particular species, or its internal structure and uses in the adult bird.

I was led to examine this process more attentively than it had been by others, in consequence of meeting with it in the snipe, in which bird it surpasses in length the cœca of the great intestines.

The appendix has an uniform structure in all the birds I have inspected for it. There appear to be but two tunics; the external is the continuation of the peritonæum; the internal of the villous coat of the intestine. The inner surface of the intestines exhibits different appearances, according to the species. In some it furnishes fine villous processes, in others zig-zag or waving laminæ, which produce a texture to the naked eye, like that of twilled cloth; but whatever may be

the arrangement of the internal coat of the intestine, the appendix is constantly found to present a surface composed of small cells, similar to those assemblages of mucous follicles that are met with in different parts of the alimentary canal. I have not observed in any instance, that the appendix received the natural contents of the adjoining intestine, but have always found it filled with a mucous fluid, I therefore conclude that it performs the office of a mucous gland.

The magnitude and form of the appendix are not so determined as its structure. In the snipe, as already mentioned, it exceeds in length the cœca of the great intestines; its width also is equal to that portion of the gut, from which it arises; but it becomes smaller towards the extremity.

In the curlew I have found it of a very considerable size, although less in proportion to the intestine than in the snipe. It is wider in the middle than at its origin or termination.

In the woodcock it is nearly as capacious as in the curlew.

I have found it very slender, but above an inch long, in the black coot. In this bird the intestines are all long and slender, and the cœca of the great intestines singularly so.

The appendix in the swan and goose is rather larger than in birds generally, and ends in a point.

It is remarkably small in the heron.

In a great number of the passerine and gallinaceous birds that I have examined for the purpose, I have found it very small.

It is short also in the hawk.

The greater size which this process of the intestine preserves in the snipe, curlew, and woodcock, would seem to depend upon the mode of feeding and habits of those birds;

although I do not perceive in what manner this peculiarity of structure becomes useful to them.

The appendix in most birds retains the marks of its origin; thus the remnant of the yolk bag is commonly found attached to its free extremity, with which it still preserves a communication. This occurs particularly in the passerine tribe, and amongst the accipitres. In the nightingale the duct is so short, that it is scarcely visible; but the yolk bag remains during life about the size of a small pea, adhering to the intestine with which it communicates.

The preceding facts furnish a curious example of the œconomy of nature, in adapting an organ of fœtal life to the exercise of a particular function in the full grown bird.

They likewise afford a proof that the yolk bag and intestine not only have communicated, but that they were originally continuous structures, a fact which has been lately denied by LEVEILLE'.

At a future period, I hope to lay before the Society, some observations upon the functions of the different parts of the egg during incubation, from which it will be found, that a communication between the yolk bag and intestines exists as a matter of necessity.

EXPLANATION OF THE FIGURES.

Fig. 1. Represents a considerable portion of the intestinal canal of the snipe, in order to show the relative size of the appendix in that bird.

a. Indicates the small intestine.

b. The great intestine, or rectum.

c. Is the appendix, on which may be seen, even upon the external surface, the marks of the cellular structure. At the extremity of the appendix, the remnant of the yolk bag may be perceived.

d. Shows the two cœca of the great intestine, which are shorter in this bird than the appendix.

Fig. 2. Is a short portion of the small intestine, with the appendix, in the curlew.

a. Is the intestine laid open, to exhibit the plicated, or twilled appearance of the villous coat which is a common structure in birds.

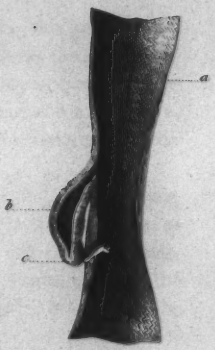
b. The appendix opened also, to expose the cellular surface internally.

c. Is the remnant of the yolk.

N. B. This figure represents the parts perhaps a little larger than natural, owing to the drawing being made from a preparation in spirits; but the proportion between the appendix and intestine are nevertheless correct.

Fig. 1.

Fig. 2.



XV. *An Account of a vegetable Wax from Brazil.* By William Thomas Brande, *Esq. F. R. S.*

Read May 9, 1811.

§. I.

THE vegetable wax described in this Paper, was given to the President by Lord GRENVILLE, with a wish, on the part of his Lordship, that its properties should be investigated, in the hope that it might prove an useful substitute for bees wax, and constitute, in due time, a new article of commerce between the Brazils and this country.

It was transmitted to Lord GRENVILLE from Rio de Janeiro, by the Comte DE GALVEAS, as a new article lately brought to that city, from the northernmost parts of the Brazilian dominions, the Capiteneas of Rio Grande and Seara, between the latitudes of three and seven degrees north: it is said to be the production of a tree of slow growth, called by the natives *Carnauba*, which also produces a gum used as food for men, and another substance employed for fattening poultry.

When the Comte wrote to Lord GRENVILLE in July last, orders had been sent to the governors of the districts where it grows, requiring them to report more particularly, on the nature and qualities of this interesting tree; we may therefore hope that information will soon be obtained, whether the article can be procured in abundance, and at a reasonable price, in which case it will become a valuable addition to the com-

forts of mankind, by reducing the price and improving the quality of candles, flambeaux, &c.

The article, in the state in which it was sent, resembles much that described by HUMBOLDT, as the produce of the Ceroxylon Andicola,* but it is not likely to be the same, as HUMBOLDT's wax is collected from a stately palm tree, which grows on the high mountains, from 900 to 1450 toises above the level of the sea, and on the edge of the regions of perpetual snow. On the other hand, the Brazilian plant is described as a slow growing tree, but not as a large one, and there are no high mountains delineated in the most accurate and recent maps of the Capiteneas where it is found. But a more decisive argument against their identity, is the analysis of VAQUELIN, published by HUMBOLDT, which shews that the produce of the Ceroxylon consists of two-thirds resin and only one-third wax; but the Brazilian article is entirely wax, and affords not the smallest trace of resin. The Brazilian plant, however, was not entirely unknown to HUMBOLDT, for it appears from his book, that M. CORREA had informed him that a palm, called *Carnauba* by the natives of Brazil, produced wax from its leaves.

§. II.

1. The wax in its rough state, is in the form of a coarse pale gray powder, soft to the touch, and mixed with various impurities, consisting chiefly of fibres of the bark of the tree, which, when separated by a sieve, amount to about 40 per cent.

It has an agreeable odour, somewhat resembling new hay, but scarcely any taste.

* Plantes Equinoctiales, p. 3.

At 206° FAHRENHEIT, it enters into perfect fusion, and in this state it may be further purified, by passing it through fine linen. By this process, it acquires a dirty green colour, and its peculiar smell becomes more evident. When cold, it is moderately hard and brittle. Its specific gravity is ,980.

2. Water exerts no action on the wax, unless boiled with it for some hours; it then acquires a slight brown tinge, and the peculiar odour of the wax.

3. Alcohol does not dissolve any portion of the wax, unless heat be applied.

Two fluid ounces of boiling alcohol, spec. grav. ,826, dissolve about ten grains of the wax, of which eight grains are deposited as the solution cools, and the remaining two grains may be afterwards precipitated by the addition of water, or may be obtained unaltered by evaporating the alcohol.

The solution of the wax in alcohol has a slightly green tinge.

4. Sulphuric ether, spec. grav. .7563, dissolves a very minute portion of the wax, at the temperature of 60°.

Two fluid ounces of boiling sulphuric ether dissolve thirty grains of the wax, of which twenty-six grains are deposited by cooling the solution, and the remaining four grains may be obtained by allowing the ether to evaporate spontaneously.

5. The fixed oils very readily dissolve the wax at the temperature of boiling water, and form with it compounds of an intermediate consistence, very analogous to those which are obtained with common bees wax.

In examining some combinations which I had made of the vegetable wax with olive oil, I was surprised to find them

perfectly soluble in ether, and sparingly soluble in boiling alcohol.

As it is commonly stated that the fixed oils are insoluble in ether and in alcohol, I was led to attribute the solution of the oil, in these instances, to its being combined with the wax; but subsequent experiments, of which I shall state the general results, have shewn me that these opinions are erroneous.

Four fluid ounces of sulphuric ether, spec. grav. ,7563, dissolve a fluid ounce and a quarter of the expressed oil of almonds; of olive oil, the same quantity of the ether dissolves a fluid ounce and a half; of linseed oil, two fluid ounces and a half; and castor oil is soluble in any proportion in sulphuric ether of the above specific gravity.

The expressed oils of almonds and of olives, are very sparingly soluble in alcohol, spec. grav. ,820.

Linseed oil is more soluble than the two former. Four fluid ounces of alcohol, spec. grav. ,820, dissolve nearly one fluid drachm.

Castor oil is perfectly soluble in every proportion in alcohol, spec. grav. ,820. In alcohol of a higher specific gravity, as ,840, it is very sparingly soluble.*

As some of the difficultly soluble resins are more easily dissolved in alcohol, to which a small proportion of camphor has been added, I endeavoured to ascertain whether the fixed oils were rendered more soluble by the same means, but found that this was not the case, excepting with regard to castor oil, which although very sparingly dissolved by alcohol of a spec.

* The solubility of castor oil in alcohol was mentioned to me some months ago by Dr. WOLLASTON, who also informed me, that it had on that account been employed to adulterate certain essential oils of high value, especially the oil of cloves.

grav. above ,840°, becomes abundantly soluble, by the addition of one part of camphor, to eight parts of the alcohol.

Boiling alcohol, spec. grav. ,840, takes up a considerable portion of castor oil and of linseed oil; it also dissolves a small quantity of the oils of almonds and of olives; but they are copiously deposited during the cooling of the alcohol, and only a small portion retained in permanent solution.

When water is added to any of these solutions of the fixed oils in ether, and in alcohol, a milky mixture is formed, and the oil gradually separates upon the surface, without having undergone any apparent alteration.

6. One hundred grains of the wax were boiled for half an hour in a solution of caustic potash, spec. grav. 1090. The solution acquired a pale rose colour, but appeared to exert no further action on the wax, which after having been washed with warm water, retained its fusibility and other properties. No combination therefore, similar to a soap, was produced, nor was any precipitate occasioned by the addition of acids to the rose coloured alkaline solution.

7. The effects produced by boiling the wax in solutions of pure soda, and of the subcarbonates of soda and of potash, were analogous to those of the caustic potash.

8. Solutions of pure and of carbonated ammonia exert scarcely any action on the wax.

9. When the wax is boiled in nitric acid, spec. grav. 1,45. there is some escape of nitrous gas, and the colour of the wax is gradually changed to a deep yellow.

When the wax is removed from the acid, and washed with hot water, it is found to have become more brittle and hard, but it still retains much of its peculiar odour.

In this state it remains insoluble in the alkalies, but they

now change its colour to a very bright brown, which is destroyed by washing with dilute muriatic acid, and its original yellow colour restored.

Neither the fusibility, nor the inflammability of the wax, are impaired by this process.

Nitric acid, diluted with eight parts of water, produces the same change in the colour of the wax as the concentrated acid.

Having been unsuccessful in my attempts to bleach the wax in its original state, I made some experiments to ascertain whether its colour could be more easily destroyed, after it had been acted upon by nitric acid, and found, that by exposing it spread upon glass to the action of light, it became in the course of three weeks of a pale straw colour, and on the surface nearly white. The same change was produced, by steeping the wax in thin plates, in an aqueous solution of oxymuriatic gas, but I have not hitherto succeeded in rendering it perfectly white.

10. Muriatic acid has little action on the wax: when boiled upon it for some hours, it destroys much of its colour.

11. Sulphuric acid changes the colour of the wax to a pale brown, and when water is added, it becomes of a deep rose colour; the inflammability and the fusibility of the wax are slightly impaired by this process.

When heat is applied, the wax is decomposed with the usual phenomena, sulphurous acid is developed, and charcoal deposited.

12. Acetic acid has very little action on the wax, when cold.

When the wax is boiled in this acid, a minute portion is dissolved, and again deposited as the solution cools. By long

continued boiling in acetic acid, the wax is rendered nearly white; but when it is afterwards washed with water, and fused, it resumes its former colour.

13. When the wax is fused in oxymuriatic gas, it is rapidly decomposed, and parting with hydrogen and oxygen, muriatic acid and water are formed, and charcoal is deposited.

14. The results of the destructive distillation of the vegetable wax, are very analogous to those of bees wax.

An acid liquor, mixed with a volatile oil, are the first products; these are succeeded by a large proportion of a butyraceous oil, and a very small quantity of charcoal affording traces of lime, remains in the retort. During the process, a little carbureted hydrogene gas is given off.

I have not considered it necessary to dwell upon the relative proportions of these different products, as they will necessarily vary according to the rapidity with which the distillation is conducted.

§. III.

From the preceding detail of experiments, it appears, that although the South American vegetable wax possesses the characteristic properties of bees wax, it differs from that substance in many of its chemical habitudes; it also differs from the other varieties of wax, namely, the wax of the myrica cerifera,* of lac,† and of white lac.‡

* Vide Dr. Bostock's Experiments on the Wax of the Myrica cerifera, in Nicholson's Journal for March 1803.

† Vide Analytical Experiments and Observations on Lac, by Charles Hatchett, Esq. F. R. S. in the Philosophical Transactions for 1804.

‡ Vide Observations and Experiments on a Wax-like Substance from Madras, by George Pearson, M. D. F. R. S. in the Philosophical Transactions for 1794.

The attempts which I have made to bleach the wax, have been conducted on a small scale; but from the experiments related, it appears that after the colour has been changed by the action of very dilute nitric acid, it may be rendered nearly white by the usual means. I have not had sufficient time to ascertain whether the wax can be more effectually bleached by long continued exposure, nor have I had an opportunity of submitting it to the processes employed by the bleachers of bees wax.

Perhaps the most important part of the present inquiry, is that which relates to the combustion of the vegetable wax, in the form of candles.

The trials which have been made to ascertain its fitness for this purpose, are extremely satisfactory, and when the wick is properly proportioned to the size of the candle, the combustion is as perfect and uniform, as that of common bees wax.

The addition of from one-eighth to one-tenth part of tallow, is sufficient to obviate the brittleness of the wax in its pure state, without giving it any unpleasant smell, or materially impairing the brilliancy of its flame. A mixture of three parts of the vegetable wax, with one part of bees wax, also makes very excellent candles.

XVI. *Astronomical Observations relating to the Construction of the Heavens, arranged for the Purpose of a critical Examination, the Result of which appears to throw some new Light upon the Organization of the celestial Bodies.* By William Herschel, *LL. D. F. R. S.*

Read June 20, 1811.

A KNOWLEDGE of the construction of the heavens has always been the ultimate object of my observations, and having been many years engaged in applying my forty, twenty, and large ten feet telescopes, on account of their great space-penetrating power to review the most interesting objects discovered in my sweeps, as well as those which had before been communicated to the public in the *Connoissance des Temps*, for 1784, I find that by arranging these objects in a certain successive regular order, they may be viewed in a new light, and, if I am not mistaken, an examination of them will lead to consequences which cannot be indifferent to an inquiring mind.

If it should be remarked that in this new arrangement I am not entirely consistent with what I have already in former papers said on the nature of some objects that have come under my observation, I must freely confess that by continuing my sweeps of the heavens my opinion of the arrangement of the stars and their magnitudes, and of some other particulars, has undergone a gradual change; and indeed when the novelty of the subject is considered, we cannot be surprised

that many things formerly taken for granted, should on examination prove to be different from what they were generally, but incautiously, supposed to be.

For instance, an equal scattering of the stars may be admitted in certain calculations; but when we examine the milky way, or the closely compressed clusters of stars, of which my catalogues have recorded so many instances, this supposed equality of scattering must be given up. We may also have surmised nebulæ to be no other than clusters of stars disguised by their very great distance, but a longer experience and better acquaintance with the nature of nebulæ, will not allow a general admission of such a principle, although undoubtedly a cluster of stars may assume a nebulous appearance when it is too remote for us to discern the stars of which it is composed.

Impressed with an idea that nebulæ properly speaking were clusters of stars, I used to call the nebulosity of which some were composed, when it was of a certain appearance, *resolvable*; but when I perceived that additional light, so far from resolving these nebulæ into stars, seemed to prove that their nebulosity was not different from what I had called milky, this conception was set aside as erroneous. In consequence of this, such nebulæ as afterwards were suspected to consist of stars, or in which a few might be seen, were called *easily resolvable*; but even this expression must be received with caution, because an object may not only contain stars, but also nebulosity not composed of them.

It will be necessary to explain the spirit of the method of arranging the observed astronomical objects under consideration in such a manner, that one shall assist us to understand

the nature and construction of the other. This end I propose to obtain by assorting them into as many classes as will be required to produce the most gradual affinity between the individuals contained in any one class with those contained in that which precedes and that which follows it: and it will certainly contribute to the perfection of this method, if this connection between the various classes can be made to appear so clearly as not to admit of a doubt. This consideration will be a sufficient apology for the great number of assortments into which I have thrown the objects under consideration; and it will be found that those contained in one article, are so closely allied to those in the next, that there is perhaps not so much difference between them, if I may use the comparison, as there would be in an annual description of the human figure, were it given from the birth of a child till he comes to be a man in his prime.

The similarity of the objects contained in each class will seldom require the description of more than one of them, and for this purpose, out of the number referred to, the selected one will be that which has been most circumstantially observed; however, those who wish either to review any other of the objects, or to read a short description of them, will find their place in the heavens, or the account of their appearance either in the catalogues I have given of them in the Philos. Trans. or in the *Connoissance des Temps* for 1784, to which in every article proper references will be given for the objects under consideration.

If the description I give should sometimes differ a little from that which belongs to some number referred to, it must be remembered that objects which had been observed many

times, could not be so particularly and comprehensively detailed in the confined space of the catalogues as I now may describe them; additional observations have also now and then given me a better view of the objects than I had before. This remark will always apply to the numbers which refer to the *Connoissance des Temps;* for the nebulæ and clusters of stars are there so imperfectly described, that my own observation of them with large instruments may well be supposed to differ entirely from what is said of them. But if any astronomer should review them, with such high space-penetrating-powers, as are absolutely required, it will be found that I have classed them very properly.

It will be necessary to mention that the nebulous delineations in the figures are not intended to represent any of the individuals of the objects which are described otherwise than in the circumstances which are common to the nebulæ of each assortment: the irregularity of a figure, for instance, must stand for every other irregularity; and the delineated size for every other size. It will however be seen, that in the figure referred to there is a sufficient resemblance to the described nebula to show the essential features of shape and brightness then under consideration.

1. *Of extensive diffused Nebulosity.*

The first article of my series will begin with extensive diffused nebulosity, which is a phenomenon that hitherto has not been much noticed, and can indeed only be perceived by instruments that collect a great quantity of light. Its existence, when some part of it is pointed out by objects that are within the reach of common telescopes, has nevertheless obtruded

itself already on the knowledge of astronomers, as will be seen in my third article.

The widely diffused nebulosity under consideration has already been partially mentioned in my catalogues.*

The description of the object I shall select is of No. 14 in the 5th class, and is as follows: "Extremely faint branching "nebulosity; its whitishness is entirely of the milky kind, and "it is brighter in three or four places than in the rest; the "stars of the milky way are scattered over it in the same "manner as over the rest of the heavens. Its extent in the "parallel is nearly $1\frac{1}{2}$ degree, and in the meridional direction "about 52 minutes. The following part of it is divided into "several streams and windings, which after separating, meet "each other again towards the south." See figure 1.

This account, which agrees with what will be found in all the other numbers referred to, with regard to the subject under consideration, namely, a diffused milky nebulosity, will give us already some idea of its great abundance in the heavens; my next article however will far extend our conception of its quantity.

2. *Observations of Nebulosities that have not been published before.*

It may be easily supposed that in my sweeps of the heavens I was not inattentive to extensive diffusions of nebulosity, which occasionally fell under my observation. They can only be seen when the air is perfectly clear, and when the observer

* See Phil. Trans. for 1786, page 471; for 1789, page 226; and for 1802, page 503. The following ten nebulosities are in the Vth class, No. 13, 14, 15, 17, 28, 30, 31, 33, 34, 38.

has been in the dark long enough for the eye to recover from the impression of having been in the light.

I have collected fifty-two such observations into a table, and have arranged them in the order of right ascension. In the first column they are numbered; in the second and third columns are the right ascension and north polar distance of a place which is the central point of a parallelogram comprehending the space which the nebulosity was observed to fill. They are calculated for the year 1800.

The length and breadth of the parallelograms are set down in the 4th and 5th columns in degrees and minutes of a great circle. The time taken up in the transit of each parallelogram having been properly reduced to space by the polar distance given in the 3d column, in order to make it agree with the space contained in the breadth of the zone described by the telescope; the dimensions of the former space therefore is in the parallel, and that of the latter in the meridian. My field of view, being fifteen minutes in diameter, its extent has been properly considered in the assigned dimensions of the parallelograms. It is however evident that the limits of the sweeping zone leave the extent of the nebulosity in the meridian unascertained. The beginning of it is equally uncertain, since the nebulous state of the heavens could only be noticed when its appearance became remarkable enough to attract attention. The ending is always left undetermined; for, as the right ascension was only taken once, I have allowed but a single minute of time for the extent of the nebulosity in that direction, except where the time was repeatedly taken with a view to ascertain how far it went in the parallel; or when the circumstances of its brightness pointed out a longer duration.

The sixth column of the table contains the size of the observed nebulosity reduced to square degrees and decimals, computed from the two preceding columns; and in the last I have given the account of these nebulosities as recorded in my sweeps at the time they were made; namely within a period of nineteen years, beginning in 1783 and ending in 1802.

When this account says *affected*, it is intended to mean that the ground upon which, or through which we see, or may see stars, is affected with nebulosity.

Table of extensive diffused Nebulosity.

No.	R. A.	P. D.	Paral.	Merid.	Size.	Account of the Nebulosity.
	h ′ ″	° ′	° ′	° ′	Deg.	
1	0 5 2	81 7	1 44	1 55	3,3	Much affected with nebulosity.
2	0 12 31	86 34	3 0	2 34	7,7	Much affected.
3	0 17 17	61 24	0 41	2 40	1,8	Affected.
4	0 20 31	86 34	1 30	2 34	3,6	Much affected.
5	0 25 5	67 8	0 29	2 34	1,2	Much affected.
6	0 31 22	90 4	2 30	2 19	5,7	Appeared to be affected with very faint nebulosity.
7	0 32 54	49 23	1 33	3 1	4,7	Affected with nebulosity.
8	0 34 21	51 17	1 17	2 49	3,6	Unequally affected.
9	0 36 13	47 3	2 37	3 18	8,6	Suspected faint nebulosity.
10	0 43 32	46 58	0 26	3 18	1,4	Suspected faint nebulosity.
11	1 35 32	60 42	0 28	2 40	1,3	Suspected to be tinged with milky nebulosity.
12	2 22 19	71 27	0 29	2 29	1,2	Much affected with nebulosity.
13	3 56 14	65 6	0 29	2 27	1,7	Much affected.
14	4 17 21	55 7	1 4	2 38	2,8	Suspected pretty strong nebulosity.
15	4 18 21	55 6	1 53	2 38	5,0	Suspected nebulosity.
16	4 21 35	97 44	0 30	2 15	1,1	Strong milky nebulosity.
17	4 23 14	69 23	0 29	2 36	1,3	Much affected.
18	4 38 17	69 23	0 29	2 36	1,3	Much affected.
19	4 46 17	63 25	1 46	2 31	4,4	Strong suspicion of very faint milky nebulosity.
20	5 9 44	65 6	1 23	2 27	3,4	Very much affected.
21	5 13 14	65 6	0 29	2 27	1,7	Affected.

No.	R. A.	P. D.	Parsl.	Merid.	Size.	Account of the Nebulosity.
	h ′ ″	° ′	° ′	° ′	Deg.	
22	5 23 59	97 1	2 31	2 31	6,3	Affected with milky nebulosity.
23	5 25 16	92 48	0 30	2 40	1,3	Affected.
24	5 27 2	94 23	1 48	2 32	4,6	Visible and unequally bright nebulosity. I am pretty sure this joins to the great nebula in Orion.
25	5 30 40	92 35	2 45	2 33	7,0	Diffused milky nebulosity.
26	5 31 58	97 1	1 56	2 31	4,9	A pretty strong suspicion of nebulosity.
27	5 38 5	88 55	1 6	2 37	2,9	Affected with milky nebulosity.
28	5 55 55	86 17	0 30	2 34	1,3	Much affected.
29	5 56 36	110 28	1 48	2 48	5,0	Affected.
30	6 33 7	48 39	0 26	3 4	1,3	Affected.
31	9 22 56	108 3	0 29	2 30	1,2	Affected.
32	9 27 19	18 21	0 24	4 4	1,6	Much affected with very faint whitish nebulosity.
33	10 6 56	98 33	3 58	2 17	9,1	Very faint whitish nebulosity.
34	10 16 1	37 58	0 24	4 9	1,7	Much affected.
35	10 34 29	26 44	0 29	3 15	1,6	Affected with very faint nebulosity
36	10 58 24	26 44	0 42	3 15	2,3	Affected.
37	11 56 59	58 50	0 41	2 54	2,0	Affected with whitish nebulosity.
38	12 7 34	58 50	0 41	2 54	2,0	Affected with whitish nebulosity.
39	13 7 33	55 20	0 27	2 17	1,0	Much affected.
40	13 58 0	55 20	0 42	2 17	1,6	Very much affected; and many faint nebulæ suspected.
41	15 5 7	70 40	1 52	2 31	4,7	Affected with very faint nebulosity.
42	20 58 20	92 17	1 45	2 21	4,1	Much affected with whitish nebulosity.
43	20 48 50	73 38	0 29	2 52	1,4	A good deal affected.
44	20 51 4	46 51	0 59	2 53	2,8	Faint milky nebulosity scattered over this space, in some places pretty bright.
45	20 52 28	91 57	0 49	0 56	0,8	Much affected with whitish nebulosity.
46	20 53 31	47 7	1 8	3 18	3,7	Suspected nebulosity joining to plainly visible diffused nebulosity.
47	21 0 26	76 3	0 44	2 46	2,0	Affected.
48	21 29 27	80 8	0 30	2 15	1,1	Much affected.
49	21 42 16	68 57	0 29	2 36	1,2	Affected.
50	22 52 36	64 47	0 29	2 47	1,3	Much affected.
51	22 53 6	64 47	0 42	2 47	1,9	Affected.
52	22 55 29	61 15	0 28	2 37	1,2	A little affected.

In looking over this table, it may be noticed that I have inserted several nebulosities that were only suspected. Had I been less scrupulous at the time of observation the word suspected would generally have been omitted; for with this nebulosity, as well as with the great number of nebulæ that in my catalogues are marked suspected, I have almost without exception found, in a second review, that the entertained suspicion was either fully confirmed, or that, without having had any previous notice of the former observation, the same suspicion was renewed when I came to the same place again.

When these observations are examined with a view to improve our knowledge of the construction of the heavens, we see in the first place that extensive diffused nebulosity is exceedingly great indeed; for, the account of it, as stated in the table, is 151,7 square degrees; but this, it must be remembered, gives us by no means the real limits of it, neither in the parallel nor in the meridian; moreover the dimensions in the table give only its superficial extent; the depth or third dimension of it may be far beyond the reach of our telescopes; and when these considerations together are added to what has been said in the foregoing article, it will be evident that the abundance of nebulous matter diffused through such an expansion of the heavens must exceed all imagination.

By nebulous matter I mean to denote that substance, or rather those substances which give out light, whatsoever may be their nature, or of whatever different powers they may be possessed.

Another remark of equal importance arises from the consideration of the observed nebulosities. By the account of the table we find that extreme faintness is predominant in most

of them; which renders it probable that our best instruments will not reach so far into the profundity of space, as to see more distant diffusions of it. In No. 44 of the table, we have an instance of faint milky nebulosity, which, though pretty bright in some places, was completely lost from faintness in others; and No. 46 confirms the same remark. It has also been already mentioned in the first article, that the nebulosity in V. 14 was brighter in three or four places than in the rest. The stars also of the milky way which were scattered over it, and were generally very small, appeared with a brilliancy that will admit of no comparison with the dimness of the brightest nebulosity. In consequence of this, we may already surmise that the range of the visibility of the nebulous matter is confined to very moderate limits.

3. *Of Nebulosities joined to Nebulæ.*

The nature of diffused nebulosity is such that we often see it joined to real nebulæ; for instances of this kind we have the fourteen following objects.*

The account of the three first nebulæ being shortened in the catalogue, I give it here more at length.

No. 81 in the first class is "A considerable bright and large "nebula. Its nebulosity is of the milky kind, and a small "part of it is considerably brighter than the rest. The great-"est extent of the milkiness is preceding the bright part, and "the termination of it is imperceptible." To No. 207 should be added "It seems to join to imperceptible nebulosity on the "south preceding side;" and to No. 214, "It terminates

* See I. 81, 207, 214. IV. 41. V. 32, 35, 37, 44, 51, 52. *Connoissance des Temps* 17, 42, 64, 78.

"abruptly to the north and is diffused to the south." See fig. 2.

No. 42 of the *Connoissance* is the great nebula in the constellation of Orion discovered by HUYGHENS. This highly interesting object engaged my attention already in the beginning of the year 1774, when viewing it with a NEWTONIAN reflector I made a drawing of it, to which I shall have occasion hereafter to refer; and having from time to time reviewed it with my large instruments, it may easily be supposed that it was the very first object to which, in February 1787, I directed my forty feet telescope. The superior light of this instrument shewed it of such a magnitude and brilliancy that, judging from these circumstances, we can hardly have a doubt of its being the nearest of all the nebulæ in the heavens, and as such will afford us many valuable informations. I shall however now only notice that I have placed it in the present order because it connects in one object the brightest and faintest of all nebulosities, and thereby enables us to draw several conclusions from its various appearance.

The first is that the extensive diffused nebulosities contained in the objects of the preceding articles are of the same nature with the nebulosity in this great nebula; for when we pursue it in its extensive course it assumes precisely the same appearance as the before-mentioned diffused nebulosities.

The second consequence we may draw from the circumstance of its containing both the brightest and faintest nebulosity joined in one object is a confirmation of an opinion already conceived in the second article, namely, that the range of the visibility of nebulous matter is what may be called very limited. The depth of the nebula may undoubtedly be ex-

ceedingly great, but when we consider that its greatest brightness does not equal that of small telescopic stars, as may be seen by comparing four of them situated within the inclosed darkness of the nebula, and several within its brightest appearance, with the intensity of the nebulous light; it cannot be expected that such nebulosities will remain visible when exceedingly farther from us than this prime nebula: the ratio of the known decrease of light will not admit of a great range of visibility within the narrow limits whereby this shining substance can affect the eye.

From this argument a secondary conclusion may be drawn, which adds to what has already been said in the foregoing article, namely, that if our best telescopes cannot be expected to reach the nebulous matter, which by analogy we may suppose to be lodged among the very small stars plainly to be seen by them; the actual quantity of its diffusion may still farther exceed even the vast abundance of it already proved to exist. A nebulous matter, diffused in such exuberance throughout the regions of space, must surely draw our attention to the purpose for which it probably may exist; and it must be the business of a critical inquirer to attend to all the appearances under which it will be exposed to his view in the following observations.

4. *Of detached Nebulosities.*

The nebulosities of the preceding articles are not restricted to an extensive diffusion; we meet with them equally in detached collections; I shall only mention the following six.*

V. 21 consists of "A broad faint nebulosity extended in the

* See I. 92. V. 21, 26, 36, 41, 42.

" form of a parallelogram with a short ray from the preced-
" ing corner towards the south. The nebulosity is nearly of
" an equal brightness throughout the parallelogram, which is
" about 8′ long and 5 or 6′ broad, but ill defined." See fig. 3, *a*, *b*, *c*.

5. *Of milky Nebulæ.*

When detached nebulosities are small we are used to call them nebulæ, and it is already known from my catalogues that their number is very great. It will therefore be sufficient to refer only to a few, of which the nebulosity is of the milky kind.*

No. 9 in the 5th class is " A large, extended, broad, faint
" nebula; its nebulosity, like that of the preceding one (which
" is De la Caille's last but one in the Catalogue des Nebu-
" leuses du Ciel Austral†) is of the milky kind."

The only purpose for which the nebulæ of these two classes have been placed in this connection, is to show that large detached nebulosities, whatever may be their appearance, as well as those nebulæ expressly called milky, partake of the general nature of the diffused nebulous matter, pointed out in the preceding articles.

6. *Of milky Nebulæ with Condensation.*

In looking at the beautiful nebula in Orion; to which I refer, because every common good telescope will shew it sufficiently well for the present purpose; we perceive that it is not equally bright in all its parts, but that its light is more

* See I. 204, III. 1, 116. IV. 7, 20, 30. V. 9, 25.

† See Connoissance des Temps for 1784, page 272.

condensed in some places than in others. The idea of condensation occurs so naturally to us when we see a gradual increase of light, that we can hardly find a more intelligible mode of expressing ourselves than by calling it condensed. The numerous instances that will be given hereafter of nebulæ that have this kind of condensation, renders it unnecessary to refer to more than the following four.*

The first of these, No. 11 in the first class, is "A bright "nebula of some extent, although not very large. It is of an "irregular figure, and the greatest brightness lies towards "the middle. The whitishness of this nebula is of the milky "kind." See figure 4.

By attending to the circumstances of the size and figure of this nebula, we find that we can account for its greater brightness towards the middle in the most simple manner by supposing the nebulous matter of which it is composed to fill an irregular kind of solid space, and that it is either a little deeper in the brightest place, or that the nebulosity is perhaps a little more compressed. It is not necessary for us to determine at present to which of these causes the increase of brightness may be owing; at all events it cannot be probable that the nebulous matter should have different powers of shining such as would be required independent of depth or compression.

7. *Of Nebulæ which are brighter in more than one Place.*

It is not an uncommon circumstance that the same nebula is brighter in several different places than in the rest of its compass. The following six are of this sort.†

* See I. 11, 84. III. 457. IV. 12.

† See I. 165, 213, 261. II. 297, 406. III. 491

No. 213 in the first class is "A very brilliant and consider-" ably large nebula, extended in a direction from south pre-" ceding to north following. It seems to have three or four " bright nuclei." See fig. 5.

From this construction of the nebula, we may draw some additional information concerning the point which was left undetermined in my last article; for since there it was proposed as an alternative, that the nebulous matter might either be of a greater depth or more compressed in the brightest part of the nebula then under consideration, we have now an opportunity to examine the probability of each case. If here the appearance of several bright nuclei is to be explained by the depth of the nebulous matter, we must have recourse to three or four separate very slender and deep projections, all situated exactly in the line of sight; but such a very uncommon arrangement of nebulous matter cannot pretend to probability; whereas a moderate condensation, which may indeed be also accompanied with some little general swelling of the nebulous matter about the places which appear like nuclei, will satisfactorily account for their superior brightness.

The same method of reasoning may be as successfully applied to explain the number of unequally bright places in the diffused nebulosities which have been described in the 1st, 2d, and 3d articles. For instance, in the branching nebulosity V. 14, we find three or four places brighter than the rest—in the nebulosity No. 44 of the table we have places of different brightness. In the nebula of Orion, there are many parts that differ much in lustre; and in V. 37 of the same article I found, by an observation in the year 1790, the same variety of appearance. In all these cases a proportional condensation

of the nebulous matter in the brighter places will sufficiently account for their different degree of shining.

This way of explaining the observed appearances being admitted, it will be proper to enter into an examination of the probable cause of the condensation of the nebulous matter. Should the necessity for such a condensing cause be thought to be admitted upon too slight an induction, a more detailed support of it will hereafter be found in the condition of such a copious collection of objects, as will establish its existence beyond all possibility of doubt.*

Instead of inquiring after the nature of the cause of the condensation of nebulous matter, it would indeed be sufficient for the present purpose to call it merely a condensing principle; but since we are already acquainted with the centripetal force of attraction which gives a globular figure to planets, keeps them from flying out of their orbits in tangents, and makes one star revolve around another, why should we not look up to the universal gravitation of matter as the cause of every condensation, accumulation, compression, and concentration of the nebulous matter? Facts are not wanting to prove that such a power has been exerted; and as I shall point out a series of phenomena in the heavens where astronomers may read in legible characters the manifest vestiges of such an exertion, I need not hesitate to proceed in a few additional remarks on the consequences that must arise from the admission of this attractive principle.

The nebula, for instance, which has been described at the beginning of this article, as containing several bright nuclei, has probably so many predominant seats of attraction, arising

* See Article 24.

from a superior preponderance of the nebulous matter in those places; but attraction being a principle which never ceases to act, the consequence of its continual exertion upon this nebula will probably be a division of it, from which will arise three or four distinct nebulæ. In the same manner its operation on the diffused nebulosities that have many different bright places, will possibly occasion a breaking up of them into smaller diffusions and detached nebulæ; but before I proceed with conjectures, let us see what observations we have to give countenance to such expectations.

8. *Of double Nebulæ with joined Nebulosity.*

In addition to the instances referred to in the preceding article, of nebulæ that have more than one centre of attraction I give the following list of what may be called double nebulæ.*

The 316th nebula in the second class to which in the catalogue is joined the 317th, consists of "two small faint nebulæ "of an equal size within 1′ of each other. Each has a seem-"ing nucleus, and their apparent nebulosities run into each "other. Their relative position is in a direction from south "preceding to north following." See fig. 6, *a* and *b*.

Each of the fifteen objects referred to contains two nuclei or centers of attraction, and if the active principle of condensation carries on its operation, a division of their at present united nebulosities must, in the end, be the consequence. I

* See I. 56, 176, 178, 193. II. 80, 271, 309, 316, 832. III. 45, 644. IV. 8, 28. *Connoiss.* 27, 51.

have given two figures for the same double nebulæ. For, although the nebulosities of figure *b*, when seen in the direction of the dotted lines will appear to run together, they may nevertheless be at some small distance from each other; but the same cause which will bring on a separation of it in figure *a* will also make two distinct nebulæ of figure *b*.

With regard to their being double nebulæ, it may be objected that this double appearance may be a deception; and indeed if this were a double star, instead of a double nebula, there might be some room for such a surmise. But on two accounts the case is very different. In the first place, we have not nebulæ without number at all distances to which we might have recourse, in supposing one to be far behind the other, as we have stars behind stars to produce an appearance of their being double. In the next; if what has been said of the confined range of the visibility of the nebulous matter be recollected, especially where it is so faint as in the double nebula which has been described, we cannot harbour an idea that the two objects of which it is composed are very far asunder. Add to this their great resemblance in size, in faintness, in nucleus, and in their nebulous appearance; from all which I believe it must be evident that their nebulosity has originally belonged to one common stock.

9. *Of double Nebulæ that are not more than two Minutes from each other.*

To add to the probability of the separation of nebulæ, we ought to have a considerable number of them already sepa-

rated. The following twenty-three are completely divided although not more than two minutes from one another.*

A description of II. 714 is "Two pretty bright nebulæ; "they are both round, small, and about 2′ from each other, "in a meridional direction."

Of III. 755 is "Two very faint, very small extended ne- "bulæ within $1\frac{1}{2}'$ from each other."

That all these nebulæ are really double, is founded on the reason already assigned in the last article. Then if we would enter into some kind of examination how they came to be arranged into their binary order, we cannot have recourse to a promiscuous scattering, which by a calculation of chances can never account for such a peculiar distribution of them. If, on the contrary, we look to a division of nebulous matter by the condensing principle, then every parcel of it, which had more than one preponderating seat of attraction in its extent, must in the progress of time have been divided.

No doubt can be suggested on account of the great length of time such a division must have taken up, when we have an eternity of past duration to recur to.

10. *Of double Nebulæ at a greater Distance than 2′ from each other.*

It may well be supposed that more than one attractive center would not be so frequent a case in small distances, as in nebulosities of a more extended compass; accordingly we find that separated nebulæ at more than 2′ from each other are much

* See I. 116, 190, 197. II. 8, 28, 57, 111, 178, 450, 714. III. 92, 228, 280, 591, 687, 719, 755, 855, 886, 943, 952, 959, 967.

more numerous. The following 101 double nebulæ referred to will confirm this statement.*

No. 36 and 37 in the first class are "Two small bright "nebulæ, both a little extended."

No. 74 and 75 in the second class are "Two pretty bright "nebulæ; the preceding of them is almost round; the fol- "lowing very much extended in length; they are not far "from the same parallel, and about 8 or 10′ distant from each "other."

No. 127 and 128 in the third class are "Two extremely "faint nebulæ, about 3′ from each other, and nearly in the "same parallel. The second is a very little brighter than the "first, and is of an irregular round figure."

It is remarkable that in the description of all these 101 nebulæ, there are not more than five or six which differ so much in brightness from one another, that we could suppose them to be at any considerable different distance from us; and equal brightness or faintness runs through them all in general; but supposing that any two nebulæ should even differ as much from one another, as the set of the first class which has been described, is different from the faintness of the last described set, yet this would not nearly amount to the difference in the brightness of one part of the nebula in Orion from that of another of the same nebula.

* See I. 28, 36, 90, 145. II. 17, 44, 55, 61, 74, 84, 85, 115, 118, 121, 139, 153, 167, 219, 228, 233, 333, 388, 426, 429, 455, 518, 546, 550, 580, 614, 679, 684, 692, 751, 764, 787, 789, 841, 842, 865, 868. III. 9, 15, 35, 44, 51, 62, 97, 117, 121, 127, 129, 138, 154, 159, 162, 166, 167, 172, 196, 199, 210, 216, 231, 250, 277, 306, 323, 335, 344, 351, 377, 402, 404, 407, 416, 422, 431, 511, 546, 551, 572, 574, 592, 629, 635, 657, 678, 707, 758, 781, 798, 800, 802, 807, 869, 897, 917, 957, 959, 974.

11. *Of treble, quadruple, and sextuple Nebulæ.*

If it was supposed that double nebulæ at some distance from each other would frequently be seen, it will now on the contrary be admitted that an expectation of finding a great number of attracting centers in a nebulosity of no great extent is not so probable; and accordingly observation has shewn that greater combinations of nebulæ than those of the foregoing article are less frequently to be seen. The following-list however contains 20 treble, 5 quadruple, and 1 sextuple nebulæ of this sort.*

Among the treble nebulæ there is one, namely V. 10, of which the nebulosity is not yet separated. "Three nebulæ "seem to join faintly together, forming a kind of triangle; "the middle of which is less nebulous, or perhaps free from "nebulosity; in the middle of the triangle is a double star of "the 2d or 3d class; more faint nebulosities are following."

Among the quadruple nebulæ we have III. 358. "Four "nebulæ, all within three minutes. The largest is faint and "small; the other three are less and fainter. They form a "small quartile, the largest being the most north of the pre-"ceding side."

"The nebulæ which form the sextuple one are all very "faint and very small; they take up a space of more than "10 or 12 minutes."

* See *treble nebulæ*. I. 17. II. 50, 123, 141, 171, 215, 392, 447. III. 85, 94. 117, 156, 300, 358, 382, 592, 873, 900, 945. V. 10.
Quadruple. II. 482, 568. III. 356, 358, 562.
Sextuple. III. 391.

12. *Of the remarkable Situation of Nebulæ.*

The number of compound nebulæ that have been noticed in the foregoing three articles being so considerable, it will follow, that if they owe their origin to the breaking up of some former extensive nebulosities of the same nature with those which have been shewn to exist at present, we might expect that the number of separate nebulæ should far exceed the former, and that moreover these scattered nebulæ should be found not only in great abundance, but also in proximity or continuity with each other, according to the different extents and situations of the former diffusions of such nebulous matter. Now this is exactly what by observation, we find to be the state of the heavens.

In the following seven assortments we have not less than 424 nebulæ; some of them of unascertained size, figure, or condensation; and the rest with only the first of these three essential features recorded.

The reason for not having a more circumstantial account of such a number of objects, is that they crowded upon me at the time of sweeping in such quick succession, that of sixty-one I could but just secure the place in the heavens, and of the remaining three hundred and sixty-three, I had only time to add the relative size.*

* See *sixty-one nebulæ.* II. 30, 66, 68, 70, 109, 114, 117, 125, 138, 170, 174, 176, 345, 361, 390, 391, 496, 499, 541, 542, 543, 572, 573, 629, 631, 806, 898. III. 20, 26, 31, 33, 39, 41, 42, 89, 103, 189, 193, 205, 332, 353, 363, 364, 365, 390, 413, 432, 481, 482, 483, 484, 485, 669, 670, 705, 796, 819, 930, 934, 936. *Connoiss.* 84.

Ten extremely small nebulæ. III. 98, 108, 194, 195, 230, 238, 297, 526, 545, 639.

One hundred and thirty-six very small nebulæ. II. 22, 64, 67, 72, 91, 93, 287,

Neither of the nebulæ in these seven divisions will require a description, as the title of each assortment contains all that has been ascertained about them; but their number and situation, especially when added to those that will be contained in the following articles, completely supports what has been asserted, namely, that the present state of the heavens presents us with several extensive collections of scattered nebulæ, plainly indicating by their very remarkable arrangement, that they

354, 367, 464, 497, 527, 544, 640, 641, 675, 720, 724, 739, 876. III. 6, 13, 22, 24, 34, 37, 38, 104, 111, 140, 164, 166, 186, 190, 237, 247, 255, 283, 285, 302, 303, 304, 309, 315, 317, 319, 325, 326, 333, 338, 339, 343, 354, 385, 386, 387, 389, 398, 411, 412, 421, 425, 430, 433, 435, 437, 443, 444, 453, 459, 460, 467, 470, 501, 507, 509, 525, 539, 544, 578, 579, 607, 618, 623, 625, 634, 638, 640, 641, 645, 650, 652, 659, 666, 702, 704, 708, 716, 718, 731, 733, 738, 762, 766, 775, 787, 788, 789, 799, 803, 809, 827, 831, 833, 836, 837, 838, 839, 848, 849, 866, 875, 883, 884, 894, 895, 905, 912, 913, 919, 956, 960, 961, 962, 965, 966.

Forty-two not very small nebulæ. I. 119. II. 65, 73, 100, 163, 248, 327, 352, 375, 382, 472, 606, 639, 765, 821, 838. III. 17, 30, 249, 281, 321, 327, 366, 375, 504, 548, 615, 628, 647, 660, 667, 698, 712, 715, 734, 751, 773, 774, 840, 850, 941. *Connois.* 89.

One hundred and seven small nebulæ. I. 25, 123. II. 18, 42, 46, 60, 71, 92, 94, 169, 264, 294, 324, 343, 350, 351, 356, 363, 374, 379, 381, 395, 396, 397, 398, 441, 493, 512, 529, 530, 559, 577, 578, 678, 710, 743, 778, 779, 794, 800. III. 25, 48, 57, 59, 60, 69, 74, 192, 206, 235, 243, 308, 328, 329, 334, 337, 350, 380, 420, 446, 458, 462, 464, 475, 478, 502, 516, 517, 529, 550, 588, 611, 651, 661, 664, 668, 721, 722, 723, 729, 761, 763, 769, 779, 780, 794, 797, 814, 826, 833, 841, 843, 861, 880, 881, 894, 915, 924, 925, 926, 927, 928, 939, 950, 951, 954, 969.

Fifty-eight pretty large nebulæ. I. 22, 24, 85, 169, 283. II. 34, 83, 107, 119, 137, 146, 296, 342, 358, 362, 366, 380, 383, 384, 385, 386, 387, 419, 498, 630, 652, 670, 713, 748, 801, 844, 862, 903, 905. III. 14, 18, 40, 70, 75, 76, 102, 213, 261, 279, 318, 340, 367, 372, 374, 415, 454, 473, 503, 543, 599, 662, 790, 970.

Ten large nebulæ. II. 106, 120, 175, 176. III. 28, 361, 440, 480. V. 6. *Connoiss.* 58.

owe their origin to some former common stock of nebulous matter.

To refer astronomers to the heavens for an inspection of these and the following nebulæ, would be to propose a repetition of more than eleven hundred sweeps to them, but those who wish to have some idea of the nebulous arrangements may consult Mr. BODE's excellent Atlas Cœlestis. A succession of places where the nebulæ of my catalogues are uncommonly crowded, will there be seen beginning over the tail of Hydra and proceeding to the southern wing, the body and the northern wing of Virgo, Plate 14. Then to Coma Berenices, Canes venatici, and the preceding arm of Bootes, Plate 7. A different branch goes from Comâ Berenices to the hind legs of Ursa major. Another branch passes from the wing of Virgo to the tail and body of Leo, Plate 8.

It will not be necessary to point out many other smaller collections which may be found in several plates of the same Atlas.

On the other hand, a very different aspect of the heavens will be perceived when we examine the following constellations. Beginning from the head of Capricorn, Plate 16, thence proceeding to Antinous, to the tail of Aquila, Plate 9, to Ramus Cerberus, and the body of Hercules, Plate 8, to Quadrans Muralis, Plate 7, and to the head of Draco, Plate 3. We may also examine the constellations of Auriga, Lynx, and Camelopardalus, Plate 5.

In this second review, it will be found that here the absence of nebulæ is as remarkable, as the great multitude of them in the first mentioned series of constellations.

13. *Of very narrow long Nebulæ.*

In order to advance in our knowledge of the condition of the nebulous matter, we may investigate the form of its expansion by the figure of the nebulæ that have been observed. The following five are particular instances of some that were much extended in length, but very little in breadth.*

No. 254 in the 3d class is "A very faint nebula, extended "from north-preceding to south-following. It is about 5′ "long and less than ¼ minute broad." See fig. 7.

The expansion of the nebulous matter in general may be considered as consisting of three dimensions; these may all be either nearly equal, or one of them may be much less than the other two; or the extent of two of them may be very inferior to that of the third. The nebulæ which have now been referred to exclude a nebulosity of three nearly equal dimensions, which can never be seen under less than two of them. When two of the dimensions of the nebulous matter are nearly equal, one of them may indeed be only visible; but then the chance that the other should be exactly parallel to the line of sight, is by no means favourable. The most plausible way of accounting for the apparent figure of these nebulæ is, therefore, to admit that the expansion of the nebulosity consists indeed of a very narrow length, and not much depth. This form when ascribed to nebulous matter, is sufficiently uncommon for us to expect to see many nebulæ of the figure of extended rays.

* See I. 23, 206. III. 254. IV. 72. V. 20.

14. *Of extended Nebulæ.*

This class of nebulæ, which are chiefly extended in length, but at the same time have a considerable breadth, is very numerous. I have divided the nebulæ it contains, which are 284, into five assortments as follows.*

II. 514 is "A faint nebula extended from south-preceding "to north-following; it is about 2′ long and 1′ broad." See fig. 8.

III. 523 is "A very faint nebula extended from south-pre-"ceding to north-following; it is 3 or 4′ long and nearly 3′ "broad."

* See *one hundred and sixty-one extended nebulæ of various small sizes.* I. 80, 89, 194, 202, 234. II. 14, 53, 72, 82, 108, 133, 145, 164, 206, 260, 262, 278, 280, 305, 348, 414, 436, 437, 486, 507, 520, 522, 574, 585, 611, 627, 638, 642, 649, 668, 682, 696, 700, 723, 731, 742, 772, 785, 786, 802, 809, 810, 826, 830, 831, 835, 837, 844, 847, 853, 859, 885. III. 4, 23, 56, 58, 65, 66, 73, 79, 82, 100, 110, 132, 183, 218, 225, 236, 241, 242, 244, 248, 258, 265, 305, 313, 314, 316, 342, 347, 348, 355, 369, 370, 406, 410, 419, 427, 429, 441, 442, 445, 450, 479, 487, 490, 494, 496, 499, 510, 514, 515, 520, 521, 528, 554, 557, 567, 569, 570, 586, 598, 599, 601, 612, 613, 619, 646, 649, 653, 677, 681, 682, 713, 714, 727, 730, 732, 752, 767, 771, 778, 783, 792, 804, 806, 808, 811, 812, 813, 816, 832, 845, 846, 874, 885, 892, 904, 914, 920, 929, 932, 942, 948, 949, 973.

Sixty-two extended nebulæ of various large sizes. I. 14, 20, 76, 141, 189, 212, 215, 220, 253. II. 3, 17, 23, 63, 113, 126, 134, 147, 152, 156, 165, 188, 221, 235, 251, 300, 326, 335, 344, 355, 378, 407, 453, 492, 525, 548, 566, 579, 595, 607, 619, 628, 671, 687, 703, 750, 755, 762, 799. III. 253, 282, 290, 346, 414, 492, 498, 508, 610, 689, 740, 766, 776, 921.

Thirty one extended nebulæ from ½ to 2′ long. II. 150, 181, 222, 237, 365, 479, 510, 514, 535, 582, 624, 654, 655, 674, 763, 798, 807, 829, 881, 897, 899, 901. III. 203, 368, 506, 556, 620, 648, 692, 906, 907.

Twenty-four extended nebulæ from 2 to 5′ long. I. 94, 174, 201. II. 227, 284, 291, 402, 432, 490, 536, 558, 600, 664, 747, 784, 900. III. 362, 523, 524, 553, 603, 710, 711, 717.

Six extended nebulæ from 5 to 15′ long. I. 134, 153, 285. II. 824. V. 5, 23.

I. 134 is " A considerably bright nebula, 7 or 8 minutes long " and about 3′ broad."

The considerable breadth of these nebulæ, although chiefly extended in length, proves that two of the dimensions of the nebulous matter, namely, the breadth and depth, are probably not very different; for if the depth, which is the dimension we do not see, should be equal to the length, the chance of its being out of sight is not sufficiently probable to happen very frequently. It is therefore to be supposed that the extension in length is really the greatest; for as we actually see it under this form, we are assured that it is at least as long as it appears, whereas one of the other dimensions, if not both, must certainly be less than the length. This kind of expansion admits of the utmost variety of lengthened form and position; and from the great number of nebulæ to which I have referred, the existence of such nebulosities is fairly to be deduced.

15. *Of Nebulæ that are of an irregular Figure.*

Among the various figures that may be seen in nebulæ we have a great many that are of an irregular appearance; I have divided the following ninety-three into two assortments.*

I. 61 is " A very bright small nebula north-following a

* See *sixty-one irregular nebulæ of various small sizes.* I. 61, 284. II. 185, 242, 259, 274, 281, 306, 339, 415, 445, 586, 597, 601, 605, 647, 744, 761, 834, 886, 893, 907. III. 12, 83, 191, 259, 273, 287, 301, 310, 456, 465, 485, 486, 493, 495, 533, 535, 537, 555, 581, 582, 605, 642, 663, 675, 699, 701, 724, 735, 795, 817, 834, 847, 851, 868, 879, 893, 963, 976, 977.

Thirty-two irregular nebulæ of various large sizes. I. 138, 246, 248, 282. II. 43, 81, 149, 289, 346, 349, 360, 421, 467, 468, 495, 587, 651, 681, 711, 749, 756, 804, 877. III. 137, 257, 274, 463, 683, 695, 765, 911, 938.

" star of the 9th magnitude. It is of an irregular figure." See fig. 9.

II. 289 is " A faint pretty large nebula; it is of an irregular " triangular figure."

By calling the figure of a nebula irregular, it must be understood that I saw no particular dimension of it sufficiently marked to deserve the name of length; for had there been such a distinction, its extension in the longitudinal direction would have been recorded, or, as it frequently happened, for want of time, the nebula would shortly have been called extended. From this consideration it follows, that the nebulous matter which assumes an irregular figure when seen in a telescope, cannot be very different in two of its dimensions; and this leaving the third entirely undetermined, it may be of greater, equal, or less extent than either of the other two. But to be greater or less than the dimensions that were seen it would require the particular situation of the third dimension in either case to be in the direction of the line of sight, which is so far at least improbable, that we may fairly suppose the unseen dimension not to differ much from either of the former two.

16. *Of Nebulæ that are of an irregular round Figure.*

The apparent figure of the nebulæ contained in the foregoing articles has already assisted me in a great measure to assign the expanded form of the nebulous matter of which they consist. The irregular round appearance of the following fifty-five nebulæ however, being of a much more marked description than the former, will lead to more decisive conclusions. I have divided them into three assortments.*

* See *twenty-eight nebulæ of an irregular round figure of various small sizes.* I.

No. 177 in the third class is "A very faint nebula of an "irregular round figure, about 2 or 3 minutes in diameter." See fig. 10.

The appearance of an irregular round figure necessarily requires that the extent of two dimensions of the nebulous matter should be nearly equal in every direction at right angles to each other. The unseen dimensions may certainly be longer or shorter than the visible irregular diameter; but then it must be absolutely extended centrally in the line of sight, which is a condition that has no probability in its favour; and the greater the number is, of such nebulæ, the less is the probability that the form of the nebulous matter should be irregularly cylindrical, or conical. For, except an irregular cylinder or cone, placed in the particular required situation, no expansion of the nebulous matter but an irregular globular one can be the cause of the irregular round figure of the above-mentioned nebulæ. Then since the irregular globular form has this advantage above the cylindrical and conical figure, that it will answer the required end in any situation whatsover, it is certainly that which ought to be admitted as the cause of the observed appearance.

This method of reasoning upon the form of the nebulous matter from the observed figure of nebulæ, will lead us a step farther than it might have been supposed. For granting it to

231. II. 97, 191, 243, 254, 273, 336, 560, 758, 895, 896. III. 208, 224, 311, 474, 566, 600, 614, 621, 673, 674, 688, 728, 784, 813, 835, 931, 955.

Twenty-one nebulæ of an irregular round figure of various large sizes. I. 69, 108, 161. II. 197, 240, 494, 513, 537, 538, 552, 685, 727, 872, 890. III. 426, 447, 558, 862, 876. V. 7. *Connoiss.* 70.

Six nebulæ of an irregular round figure of a mean diameter from 1 to 5'. III. 131, 177, 223, 261, 542, 617.

be highly probable, that the appearance of irregular round nebulæ are owing to so many irregular globular expansions of nebulous matter, it will be necessary to direct our attention to the cause which has formed this matter into such masses. To ascribe an highly improbable event to chance is not philosophical; especially as a forming cause offers itself to our view, when we direct an eye to the globular figure of the planets and satellites of the solar system.

17. *Of round Nebulæ.*

From what has been said, it appears that the figure of nebulæ is a subject of more interest than mere curiosity. The following fifty-seven were observed to be round, and I give them here in four assortments.*

As the title of each sort gives all that is necessary for the present purpose relating to the various sizes of round nebulæ, a description of one of the last will be sufficient. The observation of I. 269 says, that it is "A considerably bright round nebula of about one minute in diameter." See fig. 11.

The arguments which I have given in the foregoing article, where only nebulæ of an irregular round figure were considered, need not be repeated when a regular circular form is presented to our view; for the additional number of nebulæ,

* See *three round nebulæ.* III. 381, 511, 754.

Forty-one round nebulæ of various small sizes. I. 275. II. 54, 218, 223, 225, 329, 659, 760, 803. III. 11, 50, 78, 94, 95, 96, 149, 150, 180, 181, 209, 221, 222, 295, 371, 451, 477, 505, 622, 631, 671, 684, 726, 760, 800, 801, 810, 842, 888, 909, 946, 971.

Ten round nebulæ of various large sizes. I. 7, 124, 252. II. 19, 481, 889. III. 54, 77, 112, 452.

Three round nebulæ from 1 to 6′ in diameter. I. 269. II. 593. V. 16.

and the regularity of their figure are both greatly in favour of a conclusion, that the mass of the nebulous matter which occasions their appearance must be of a globular form.

In the last article I have only directed our attention to the cause of this very particular construction, but from the observations of the nebulæ above referred to, we may now more confidently assign the attraction of gravitation as the principle which has drawn the nebulous matter towards a center, and collected it into a spherical compass.

I have already shewn that the same principle appears to be the cause of the condensation of the nebulous matter in the bright places of nebulæ that shine with unequal degrees of light in the different parts of their extent,* and a concurrence of arguments established upon very different foundations cannot fail to give additional weight to the reasonings by which they are supoprted.

18. *Of Nebulæ that are remarkable for some particularity in Figure or Brightness.*

Among the nebulæ, which I have described as of an irregular figure, the following might have been inserted; but the real form of the nebulous matter of which they consist is probably as irregular as the figure or brightness of the nebulæ themselves. I have arranged thirty-five of them into three assortments.†

* See Article 7.

† See *two nebulæ of remarkable figure.* I. 286. V. 19.

Ten unequally bright nebulæ. I. 254. II. 200, 210, 422, 557, 591, 646. III. 142, 245, 534.

Twenty-three nebulæ that are brightest on one side. I. 113, 162. II. 26, 27, 136,

V. 19 is "A considerably bright nebula about 15′ long and "3′ broad; its length is divided in the middle by a black divi-"sion at least three or four minutes long." See fig. 12.

The nebulous matter of this nebula is probably a ring in a very oblique position with respect to the line of sight.

II. 646 is "A pretty bright, large nebula, of an irregular "figure; it is unequally bright."

The inequally of its brightness in different parts may arise from unequal condensation or from greater depth of nebulous matter.

II. 313 is "A pretty bright nebula, a little extended in the "parallel. The greatest brightness is towards the following "side, which is also the broadest; the preceding part being "more like a ray proceeding from it."

The irregular figure of these latter kind of nebulæ may be admitted to arise from the as yet imperfect concentration of a nebulous mass, in which the preponderating matter of it is not in the center.

19. *Of Nebulæ that are gradually a little brighter in the middle.*

The investigation of the form of the nebulous matter in the 13, 14, 15, and 16th articles has been founded only upon the observed figure of nebulæ; and in the 17th article the globular form of this matter deduced from the round appearance of nebulæ, has been ascribed to the action of the gravitating principle. I am now entering upon an examination of nebulæ of which, besides their figure, I have also recorded the different degrees of light, and the situation of the greatest brightness

155, 313, 332, 364, 369, 370, 442, 506, 531, 555, 589, 623. III. 120, 153, 286, 676, 700. V. 22.

with respect to their figure. These observations will establish the former conclusions by an additional number of objects, and by the decisive argument of their brightness, which points out a seat of attraction.

In the following four assortments are one hundred and fifty nebulæ, which all agree in being a little brighter in the middle. This increase of brightness must be understood to be always very gradual from the outside towards the middle of the nebula, whatever be its figure; and although this circumstance, for want of time, has often been left unnoticed in the observation, I am very sure that had the gradation of brightness been otherwise, it would certainly not have been overlooked.*

III. 859 is "A very faint small nebula; it is very gradually "a little brighter in the middle."

III. 488 is "A very faint extended nebula, near 3′ long,

* See *Thirty-two nebulæ, the particular figure of which has not been ascertained, gradually a little brighter in the middle.* II. 201, 401, 424, 444, 457, 528, 532, 616, 617, 648, 673, 677, 736, 904. III. 90, 106, 148, 331, 436, 472, 489, 519, 596, 633, 654, 655, 656, 686, 853, 860, 896, 978.

Twenty-four extended nebulæ, gradually a little brighter in the middle. II. 184, 192, 252, 285, 412, 478, 480, 565, 621, 688, 906. III. 141, 233, 449, 461, 468, 488, 532, 577, 736, 890. V. 8, 40, 50.

Twenty nebulæ of an irregular figure, gradually a little brighter in the middle. II. 213, 357, 403, 471, 487, 491, 524, 533, 594, 717, 729. III. 272, 428, 434, 626, 690, 857, 903, 947. V. 29.

Seventy-four round or nearly round nebulæ, gradually a little brighter in the middle. II. 7, 40, 102, 129, 131, 162, 190, 249, 258, 267, 276, 286, 290, 308, 320, 338, 428, 459, 474, 476, 477, 509, 516, 526, 602, 637, 699, 726, 737, 770, 780, 797, 811, 812. III. 62, 63, 94, 105, 121, 122, 123, 133, 162, 163, 252, 292, 296, 298, 330, 388, 409, 448, 466, 497, 522, 597, 608, 665, 680, 746, 750, 753, 818, 822, 823, 824, 858, 867, 889, 891, 908, 917, 918, 923.

" and above 2' broad; it is gradually a little brighter in the " middle. Fig. 13.

II. 549 is " A very large and pretty bright nebula of an " irregular figure; it is a little brighter in the middle." Fig. 14.

II. 812 is " A faint, small, round nebula; it is very gradu- " ally a little brighter in the middle, and the increase of " brightness begins at a distance from the center." Fig. 15.

It is hardly necessary to say that the united testimony of so many objects can leave no doubt about the central seat of attraction, which in every instance of figure is pointed out to be in the middle.

The only remark I have to make, relates to the exertion of the condensing power, which in the case of these nebulæ appears to have produced but a very moderate effect. This may be ascribed either to the unshapen mass of nebulous matter which would require much time before it could come to some central arrangement of form either in length, or in length and breadth, or lastly in all its three dimensions. It may also be ascribed to the small quantity of the preponderating central attractive matter; or even to the shortness of its time of acting: for in this case millions of years, perhaps are but moments.

20. *Of Nebulæ which are gradually brighter in the middle.*

By the general description of a nebula, when it is said to be gradually brighter in the middle, we are to understand that its light was observed to be obviously brighter about the center than in other parts. Had the nebulæ of this class been only a little brighter, or had they been much brighter in the middle,

such additional expressions would certainly have been used; except where time would not allow to be more particular. I have sorted two hundred and twenty-three of these nebulæ like the foregoing, according to their figure, into four classes.*

II. 409 is " A pretty bright and pretty large nebula; it is " very gradually brighter in the middle."

I. 55 is " A considerably bright, extended nebula about 4′ " long and 2′ broad, in a meridional direction; it is gradually " brighter in the middle." Fig. 16.

I. 266 is " A considerably bright, and pretty large nebula, " of an irregular figure; it is gradually brighter in the mid- " dle." Fig. 17.

I. 98 is " A considerably bright, and pretty large round " nebula; it is brighter in the middle, the brightness diminish-

* See *Thirty-nine nebulæ of an unascertained figure, gradually brighter in the middle.* I. 19, 49, 264. II. 24, 49, 87, 88, 89, 90, 319, 337, 347, 368, 373, 409, 440, 515, 534, 590, 610, 634, 636, 672, 783, 830, 840, 856, 857, 858, 860, 861, 863. III. 275, 584, 587, 602, 872, 892, 935.

Fifty extended nebulæ gradually brighter in the middle. I. 1, 55, 62, 131, 199, 241, 259, 263, 279. II. 1, 10, 52, 77, 95, 132, 135, 157, 203, 205, 211, 253, 266, 302, 325, 405, 417, 508, 539, 545, 583, 592, 613, 625, 643, 656, 667, 697, 709, 739, 773, 880, 882. III. 246, 267, 589, 594, 864, 902. V. 4, 39.

Twenty-nine nebulæ of an irregular figure, gradually brighter in the middle. I. 95, 196, 227, 266. II. 36, 56, 96, 130, 226, 265, 295, 314, 353, 423, 433, 434, 475, 488, 553, 596, 657, 663, 690, 793, 819, 825, 887. III. 397, 500.

One hundred and five round, or nearly round nebulæ, gradually brighter in the middle. I. 5, 12, 54, 70, 98, 106, 120, 148, 168, 186, 211, 222, 229, 243, 245, 274. II. 50, 51, 128, 151, 158, 160, 161, 196, 208, 224, 247, 255, 256, 263, 275, 293, 307, 312, 330, 331, 333, 359, 376, 399, 408, 411, 435, 458, 461, 465, 511, 517, 523, 562, 567, 580, 588, 594, 614, 615, 622, 632, 633, 635, 662, 712, 719, 741, 769, 777, 792, 817, 818, 845, 851, 852, 865, 866, 873, 879, 883, 884, 888, 902. III. 2, 88, 107, 138, 139, 220, 491, 527, 541, 609, 694, 739, 749, 825, 829, 865, 870, 871, 882, 899, 900, 933, 937, 940, 972.

"ing very gradually from the center towards the circum-
"ference." Fig. 18.

From the account of these nebulæ, we find again that all what has been said concerning the seat of the forming and condensing power of the nebulous matter, is abundantly confirmed by observation.

I have only to remark that, the exertion of the gravitating principle in these nebulæ, is in a more advanced state than with those of the last article; and that the same conceptions which have already been suggested, namely, the original form of the nebulous matter; its quantity in the seat of the attracting principle; and the length of the time of its action, when properly considered, will sufficiently account for the present state of these nebulæ.

21. *Of Nebulæ that are gradually much brighter in the middle.*

The nebulous matter which appears under the various forms of the following four assortments, containing two hundred and two nebulæ, assumes now a more condensed aspect, than that under which it was seen in either of the two foregoing collections; and thus by its gradually greater compression, gives us a still more decisive indication of the central seat of attraction.*

* See *Twenty-five nebulæ of unascertained figure, gradually much brighter in the middle.* I. 73. 121, 127, 140, 155, 181, 287. II. 35, 177, 187, 299, 439, 452, 540, 653, 658, 669, 686, 694, 795, 828, 855, 871. III. 863. *Connoiss.* 99.

Fifty-four extended nebulæ, gradually much brighter in the middle. I. 29, 31, 33, 35, 38, 53, 58, 64, 72, 82, 86, 93, 97, 101, 104, 125, 154, 157, 164, 184, 209, 233, 239, 274, 271, 277. II. 12, 13, 31, 37, 182, 212, 231, 282, 318, 416, 431, 463, 504, 604, 612, 626, 691, 701, 702, 704, 725, 753, 775, 875. III. 179. 198. V. 47. *Connoiss.* 49.

II. 828 is " A pretty bright small nebula, very gradually " much brighter in the middle."

I. 101 is " A considerably bright pretty large nebula, ex- " tended in the meridional direction, about 4′ or 5′ long; much " brighter in the middle." In the 40 feet telescope I saw the very gradual increase of brightness towards the middle of its length; a longer extent of the nebula was also visible. Fig. 19.

I. 219 is " A very bright considerably large nebula of an " irregular figure, very gradually much brighter in the mid- " dle." Fig. 20.

I. 63 is " A bright round nebula of about one minute in " diameter; it is much brighter in the middle, and very faint " towards the border." Fig. 21.

The greater difference between the comparative brightness of the center, and the outward parts of these nebulæ, may certainly be ascribed to the same causes that have been considered in the two foregoing articles; but in the present case, and taking into the account that this is already a third step of condensation from a little brighter to brighter; then, to much

Nineteen nebulæ of an irregular figure, gradually much brighter in the middle. I. 10, 26, 59, 66, 109, 110, 114, 115, 219, 235, 237, 276. II. 2, 20, 438, 503, 734, 827. III. 299.

One hundred and four round or nearly round nebulæ, gradually much brighter in the middle. I. 8, 16, 21, 30, 42, 63, 65, 67, 68, 74, 79, 83, 87, 88, 100, 102, 105, 111, 112, 118, 129, 135, 136, 142, 144, 147, 150, 158, 159, 166, 171, 175, 182, 185, 216, 218, 221, 232, 238, 244, 257, 260 265, 273, 278. II. 5, 11, 38, 69, 98, 148, 230, 236, 245, 250, 257, 269, 270, 277, 288, 292, 301, 303, 309, 311, 328, 418, 420, 446, 462, 466, 596, 561, 564, 575, 598, 632, 644. 645, 660, 666, 695, 707, 728, 738, 757, 767, 774, 782, 816, 823, 839, 854, 874. III. 250, 284, 512, 531, 624, 744, 859, 878. *Connoiss.* 59, 96.

brighter, there appears to be some foundation for supposing rather that this greater effect is produced by a longer time of the action of the attractive principle, than that it should arise merely from an original more favourable expansion of the nebulous matter.

22. *Of Nebulæ that have a Cometic appearance.*

Among the numerous nebulæ I have seen, there are many that have the appearance of telescopic comets. The following are of that sort.*

I. 4 is "A pretty large cometic nebula of considerable "brightness; it is much brighter in the middle, and the very "faint chevelure is pretty extensive." Fig. 22.

By the appellation of cometic, it was my intention to express a gradual and strong increase of brightness towards the center of a nebulous object of a round figure; having also a faint chevelure or coma of some extent, beyond the faintest part of the light, gradually decreasing from the center.

It seems that this species of nebulæ contains a somewhat greater degree of condensation than that of the round nebulæ of the last article, and might perhaps not very improperly have been included in their number. Their great resemblance to telescopic comets, however, is very apt to suggest the idea, that possibly such small telescopic comets as often visit our neighbourhood may be composed of nebulous matter, or may in fact be such highly condensed nebulæ.

* See *Seventeen cometic nebulæ*. I. 3, 4, 34, 217. II. 6, 15, 33, 59, 104, 153, 154, 241, 315, 404. III. 5, 21. *Connoiss.* 95.

23. *Of Nebulæ that are suddenly much brighter in the middle.*

From the third degree of visible condensation, I have in the 21st article intimated, that the length of the time of the action of the attracting principle, would explain the observed gradual accumulation of the nebulous matter. In the following eighteen nebulæ we may see a still more advanced compression of it, amounting almost to the appearance of a nucleus.*

II. 814 is " A small faint nebula, very suddenly much " brighter in the middle."

I. 39 is " A very bright nebula, extended from south-preceding to north-following, about 4′ or 5′ long, and 3′ broad; it is much brighter in the middle, but the brightness breaks off abruptly, so as almost to resemble a nucleus." Fig. 23.

I. 256 is " A very bright pretty large nebula of an irregular " figure; it is suddenly much brighter in the middle." Fig. 24.

I. 99 is " A very bright, small, round nebulæ; it is very " suddenly much brighter in the middle." Fig. 25.

From the appearance of these nebulæ, we see plainly that a progressive concentration of the nebulous matter has an existence; it is also remarkable that the condensation in long nebulæ inclines towards the shape of a nucleus, as well as in

* See *One nebula of unascertained figure, suddenly much brighter in the middle.* II. 814.

Seven extended nebulæ, suddenly much brighter in the middle. I. 39, 91, 96, 200, II. 183, 505. *Connoiss.* 66.

Two nebulæ of an irregular figure, suddenly much brighter in the middle. I. 256. II. 521.

Eight round or nearly round nebulæ, suddenly much brighter in the middle. I. 99, 138. II. 410, 413, 698. III. 251, 685. *Connoiss.* 54.

round ones, which can be ascribed only to the continued action of the attracting principle, tending to draw the nebulous extended expansion into a globular form.

A nucleus, to which these nebulæ seem to approach, is an indication of consolidation; and should we have reason to conclude that a solid body can be formed of condensed nebulous matter, the nature of which has hitherto been chiefly deduced from its shining quality, we may possibly be able to view it with respect to some other of its properties.

24. *Of round Nebulæ increasing gradually in brightness up to a Nucleus in the middle.*

It has already been proved, from the figure and central brightness of round nebulæ, that the nebulous matter of which they consist must be admitted to be of a globular form; but the following thirteen nebulæ lead me to a remark which not only applies to them, but to all the round nebulæ of the last five articles, which added to these amount to three hundred and twenty-one. They are not only round, but the gradual condensation from the circumference to the very center being of equal density of light at equal central distances, every ring or circle drawn round the center, bears witness to the existence of a central attraction. For whatever may be the intensity or ratio of the concentration at any given central distance, it follows, from the equality of brightness at the assigned distance, that no figure but a globular one can with any kind of probability explain the appearance; and that the concentration, as well as the figure, is produced by a general gravitation of the nebulous matter.*

* See I. 2, 6, 132, 151, 173, 236, 272. II. 25, 189, 716, 864. III. 518. IV. 6.

I. 151 is "A considerably bright and considerably large, "round cometic nebula; it is very gradually much brighter "in the middle, with a nucleus in the center." Fig. 26.

From the description of these nebulæ, we find that an actual nucleus has been formed in the attracting center; and that consequently a certain degree of consolidation of the nebulous matter is highly probable; for, although the quality of shining only points out the existence of something that is luminous, yet from analogy we have reason to conclude that certain material substances must be present to produce the light we perceive; and that they must be opaque, may be inferred from every thing we know about shining substances.

25. *Of Nebulæ that have a Nucleus.*

It may be expected that some considerable change will take place in the appearance of a nebula after it has come to a certain degree of continued gradual condensation. We are as yet so little acquainted with the nature and distribution of this matter, that an application of mathematical calculations, founded on the attraction of gravitation, for want of data, cannot be applied in order to suggest to us what appearance might next be expected; I shall therefore proceed in a regular manner to give the observations, which shew what these appearances are, without entering into any theoretical discussions.

In the following two assortments we have forty nebulæ.*

* See *Twenty-seven extended nebulæ, with a nucleus.* I. 43, 77, 126, 156, 170, 180, 208, 224, 240, 250, 255, 270, 280, 281. II. 238, 460, 759, 768, 796, 846, 849, 891. V. 18, 24, 48. *Connoiss.* 63, 101.

Thirteen round or nearly round nebulæ, with a nucleus. I. 107, 133, 139, 152, 167, 203, 225. II. 99, 501, 746, 754. III. 178. *Connoiss.* 90.

Number 63 of the *Connoissance des Temps* is "A very "bright nebula, extending from north-preceding to south-"following 9 or 10′ long, and near 4′ broad; it has a very "brilliant nucleus." Fig. 27.

I. 107 is "A very bright round nebula, about $1\frac{1}{2}$ minutes "in diameter; it has a bright nucleus in the middle." Fig. 28.

The nuclei of these nebulæ, after what has been proved, of the existence of a condensing power, I need not hesitate to ascribe to the longer continuance of its action, which appears to bring on a consolidation; and that this may be the consequence we may conclude, not only from the power of condensing, which argues a sufficient quantity of matter, but also from the quality of shining; for this proves that the substance which throws out the nebulous light is endowed with some other of the general qualities of matter besides that of gravitation.

A second remark I have to make is, that the opaque nature of the nebulous matter whlch was before inferred from analogy, is here supported by observation; for these consolidated nuclei have a considerable resemblance to the disks of planets; and if this matter consisted only of a luminous substance, the increase of light would probably far exceed their observed lustre: this being the case, the power of arresting light in its passage is an additional quality, very different from those which have already been mentioned, and seems to be analogous to properties which we know to belong to hard and solid bodies.

26. *Of extended Nebulæ that shew the Progress of Condensation.*

When the nebulous matter is much extended in length, it appears from the following nebulæ, that with those which

have a nucleus completely formed, the nebulosity on each side of it is comparatively reduced to a fainter state than it is in nebulæ of which the nucleus is apparently still in an incipient state. These faint opposite appendages to the nucleus I have in my observations called branches.

In some nebulæ there is also an additional small faint nebulosity of a circular form about the nucleus, and this I have called the chevelure. The following two assortments contain twenty-eight nebulæ of this kind.*

Number 65 of the *Connoissance* is " A very brilliant nebula " extended in the meridian, about 12′ long. It has a bright " nucleus, the light of which suddenly diminishes on its " border, and two opposite very faint branches." Fig. 29.

I. 205 is " A very brilliant nebula, 5′ or 6′ long and 3 or 4″ " broad; it has a small bright nucleus with a faint chevelure " about it, and two opposite very extensive branches. Fig 30.

The construction of these nebulæ is certainly complicated and mysterious, and in our present state of kowledge it would be presumptuous to attempt an explanation of it; we can only form a few distant surmises, which however may lead to the following queries. May not the faintness of the branches arise from a gradual diminution, of the length and density of the nebulous matter contained in them, occasioned by its gravitation towards the nucleus into which it probably subsides? Are not these faint nebulous branches joining to a nucleus, upon an immense scale, somewhat like what the zodiacal light is to our sun in miniature? Does not the chevelure

* See *Twenty-three extended nebulæ with a nucleus and two opposite faint branches.* I. 9, 13, 15, 27, 32, 75, 130, 160, 163, 187, 188, 195, 223, 228, 230. II. 101, 650, 733. IV. 61. V. 43. *Connoiss.* 65, 83, 98.

Five with a nucleus, chevelure and branches. I. 194, 205, 210. V. 45. *Connoiss.* 94.

denote that perhaps some of the nebulous matter still remaining in the branches, before it subsides into the nucleus, begins to take a spherical form, and thus assumes the semblance of a faint chevelure surrounding it in a concentric arrangement? And, if we may venture to extend these queries a little farther—will not the matter of these branches in their gradual fall towards the nucleus, when discharging their substance into the chevelure, produce a kind of vortex or rotatory motion? Must not such an effect take place, unless we suppose, contrary to observation, that one branch is exactly like the other; that both are exactly in a line passing through the center of the nucleus, by way of causing exactly an equal stream of it from each branch to enter the chevelure at opposite sides; and, this not being probable, do we not see some natural cause which may give a rotatory motion to a celestial body in its very formation?

27. *Of round Nebulæ that shew the Progression of Condensation.*

When round nebulæ have a nucleus, it is an indication that they have already undergone a high degree of condensation. From their figure we are assured that the form of the nebulosity of which they are composed is now spherical, whatever may have been its original shape; and being surrounded by a chevelure, we may look upon its different evanescent degrees of faintness as a sign whereby to judge of the gradual progress of the consolidation of the nucleus. The following seventeen nebulæ are given in two assortments.*

* See *fifteen round or nearly round nebulæ, with a nucleus and faint chevelure*, I. 40, 137, 226, 242, 251, 262. II. 321. III. 291, 373. IV. 23, 54, 56, 59, 76. *Connoiss.* 32.

Two nebulæ with a nucleus and chevelure resembling nebulous stars. II. 32. III. 99.

IV. 23 is "A considerably bright nebula with a very bright nucleus, and a chevelure about 3 or 4′ in diameter." Fig. 31.

III. 99 is "A small nebula with a pretty bright nucleus and "very faint chevelure; it is almost like a nebulous star." Fig. 32.

The chevelure of these nebulæ consists probably of the rarest nebulous matter, which not having as yet been consolidated with the rest, remains expanded about the nucleus in the shape of a very extended atmosphere; or it may be of an elastic nature, and be kept from uniting with the nucleus, as their elasticity causes the atmospheres of the planets to be expanded about them. In this case we have another property of the nebulous substance to add to the former qualities of its matter.

With those nebulæ where this chevelure is uncommonly faint, and the nucleus very bright, the consolidation appears to have reached a still higher degree, and their resemblance to nebulous stars may lead to very interesting consequences.

28. *Of round Nebulæ that are of an almost uniform Light.*

The argument that the nebulous matter is in some degree opaque which is given in the 25th article, will receive considerable support from the appearance of the following nebulæ; for they are not only round, that is to say the nebulous matter of which they are composed is collected into a globular compass, but they are also of a light which is nearly of an uniform intensity except just on the borders. I give these nebulæ in two assortments.*

* See *Four from 2′ to 4′ in diameter.* IV. 50, 62, 67. *Connoiss.* 97.
Twelve nebulæ from ½ of a minute to 2′ in diameter. I. 267. II. 186, 209, 705, 836, 870. III. 152, 877. IV. 13, 14, 16, 39.

Number 97 of the *Connoissance* is "A very bright, round "nebula of about 3′ in diameter; it is nearly of equal light "throughout, with an ill defined margin of no great extent."

IV. 19 is "A pretty faint nebula of about 1′ diameter; it is "perfectly round, and of an equal light throughout; and the "edges of it are pretty well defined." Fig. 33.

Admitting that these sixteen nebulæ are globular collections of nebulous matter, they could not appear equally bright, if the nebulosity of which they are composed consisted only of a luminous substance perfectly penetrable to light; at least this could not happen unless a certain artificial condensation of it were introduced, which can have no pretension to probability in its favour. Is it not rather to be supposed, that a certain high degree of condensation has already brought on a sufficient consolidation to prevent the penetration of light, which by this means is reduced to a superficial planetary appearance?

29. *Of Nebulæ that draw progressively towards a Period of final Condensation.*

In the course of the gradual condensation of the nebulous matter, it may be expected that a time must come when it can no longer be compressed, and the only cause which we may suppose to put an end to the compression is, when the consolidated matter assumes hardness. It remains therefore to be examined, how far my observations will go to ascertain the intensity of its consolidation.

The following two assortments contain seven nebulæ, from

whose appearance a considerable degree of solidity may be inferred.*

IV. 55 is "A pretty bright round nebula, almost of an even "light throughout approaching to a planetary appearance, "but ill defined, and a little fainter on the edges; it is about "$\frac{1}{2}$ or 1 minute in diameter." Fig. 34.

IV. 37 is "A very bright planetary disk of about 35″ in "diameter, but ill defined on the edges; the center of it is "rather more luminous than the rest, and with long attention "a very bright well defined round center becomes visible." Fig. 35.

In these nebulæ we have three different indications of the compression of the nebulous matter of which they are composed: their figure, their light, and the small compass into which it is reduced. The round figure is a proof that the nebulous mass is collected into a globular form, which cannot have been effected without a certain degree of condensation.

Their planetary appearance shews that we only see a superficial lustre such as opaque bodies exibit, and which could not happen if the nebulous matter had no other quality than that of shining, or had so little solidity as to be perfectly transparent. That there is a certain maximum of brightness occasioned by condensation, is to be inferred from the different degrees of light of round nebulæ that are in a much less advanced state of compression; for these are gradually more bright towards the center; which proves that brightness keeps up with condensation till the increase of it brings on a con-

* See *Four nebulæ of a planetary appearance.* IV. 55, 60, 68, 78.
Three planetary disks with a bright central point. II. 268. IV. 37, 73.

solidation which will no longer permit the internal penetration of light, and thus a planetary appearance must in the end be the consequence; for planets are solid opaque bodies, shining only by superficial light, whether it be innate or reflected.

From the size of the nebulæ as we see them at present, we cannot form an idea of the original bulk of the nebulous matter they contain; but let us admit, for the sake of computation, that the nebulosity of the above described nebula IV. 55, when it was in a state of diffusion, took up a space of 10′ in every cubical direction of its expansion; then, as we now see it collected into a globular compass of less than one minute, it must of course be more than nineteen hundred times denser than it was in its original state. This proportion of density is more than double that of water to air.

With regard to planetary disks, which have bright central points, we may surmise that their original diffused nebulosity was more unequally scattered, and that they passed through the different stages of extended nebulæ, gradually acquiring a nucleus, chevelure, and branches. For in nebulæ of this construction, the consolidation of a nucleus is already much advanced at the time when a considerable quantity of nebulous matter, on account of its greater central distance, remains still unformed in the branches; and if the condensation of the nucleus should keep the lead, it will come to a state of great solidity and maximum of brightness by the time that the rest of the nebulosity is drawn into a planetary appearance.

30. *Of Planetary Nebulæ.*

The objects of which I shall give an account in this article have so near a resemblance to planets, that the name of planetary nebulæ very justly expresses their appearance; for notwithstanding their planetary aspect, some small remaining haziness, by which they still are more or less surrounded, evinces their nebulous origin. In my catalogues the places of the following ten have been given.*

IV. 18 is "A beautiful bright round nebula, having a pretty "well defined planetary disk of about 10 or 12″ in diameter. "It is a little elliptical, and has a very small star following, "which gives us the idea of a small satellite accompanying "its planet. It is visible in a common finder as a small star." Fig. 36.

IV. 27 is "A beautiful very brilliant globe of light, hazy "on the edges, but the haziness going off suddenly. I sup- "pose it to be from 30 to 40″ in diameter, and perhaps a very "little elliptical. The light of it seems to be all over of the "uniform lustre of a star of the 9th magnitude. The hazi- "ness on the edges does not exceed the 20th part of the "diameter."

IV. 51 is "A small beautiful planetary nebula, but consi- "derably hazy upon the edges; it is of a uniform light, and "considerably bright, perfectly round, and about 10 or 15″ in "diameter."

IV. 53 is "A pretty bright planetary nebula of nearly 1′ in "diameter; it is round, or a little elliptical; its light is uni- "form, and pretty well defined on the borders."

* See *Planetary nebulæ* IV. 1, 11, 18, 26, 27, 34, 51, 53, 64, 70.

IV. 64 is "A beautiful planetary nebula of a considerable "degree of brightness, but not very well defined, about 12 "or 15″ in diameter."

The remarks which have been made on the nebulæ of the foregoing article, will here apply with additional propriety; for the light of these planetary nebulæ must be considerably more condensed than that of the foregoing sets. The diameter of four of them does not exceed 15,″ so that if we again suppose the original diffused nebulosity from which they sprang of 10′ in cubical dimensions, we shall have a condensation, which has reduced the nebulous matter to less than the one-hundred and twenty-two thousandth part of its former bulk.

One of them, number 34 in the 4th class, appeared even in the 20 feet telescope, with the sweeping power, like a star with a large diameter, and it was only when magnified 240 times that it resembled a small planetary nebula; nor can any of these nebulæ be distinguished from the neighbouring small stars in a good common telescope, night glass, or finder.

When we reflect upon these circumstances, we may conceive that, perhaps in progress of time these nebulæ which are already in such a state of compression, may be still farther condensed so as actually to become stars.

It may be thought that solid bodies, such as we suppose the stars to be from the analogy of their light with that of our sun when seen at the distance of the stars, can hardly be formed from a condensation of nebulous matter; but if the immensity of it required to fill a cubical space, which will measure ten minutes when seen at the distance of a star of the 8th or 9th magnitude, is well considered, and properly compared with the very small angle our sun would subtend at the same

distance, no degree of rarity of the nebulous matter, to which we may have recourse, can be any objection to the solidity required for the construction of a body of equal magnitude with our sun.*

A circumstance which allies these very compressed nebulæ to the character of many of our well known celestial bodies, such as some of the planets and their satellites, the sun and all periodical stars, is that very probably most, if not all of them, turn on their axes. Seven of the ten I have mentioned are not perfectly round, but a very little elliptical. Ought we not to ascribe this figure to the same cause which has flattened the polar diameter of the planets, namely, a rotatory motion?

At the end of the 86th article I have already pointed out one configuration of the nebulous matter, of which the final condensation seems to be properly disposed for bringing on a rotatory motion of the nucleus; but, if we consider this matter in a general light, it appears that every figure which is not already globular must have eccentric nebulous matter, which in its endeavour to come to the center, will either dislodge some of the nebulosity which is already deposited, or slide upon it sideways, and in both cases produce a circular motion; so that in fact we can hardly suppose a possibility of the production of a globular form without a consequent revolution of the nebulous matter, which in the end may settle in a regular rotation about some fixed axis. Many of the extended, and irregular nebulæ are considerably elliptical, and the irregular

* A cubical space, the side of which at the distance of a star of the 8th magnitude is seen under an angle of 10″, exceeds the bulk of the sun (2208600000000000000) two trillion and 208 thousand billion times.

round ones shew a general approach to the oval form; now these figures are all favourable to a surmise, that a rotatory motion may often take place even before the nucleus of a nebula can have arrived to a state of consolidation. An objection, that this remarkable form of planetary nebulæ may be owing to chance, will hardly deserve to be mentioned, because the improbability of such a supposition must exclude it from all claim to refutation.

31. *Of the Distance of the Nebula in the Constellation of Orion.*

In my 3d article I concluded, from the appearance of the great nebula in Orion, that the range of the visibility of the diffused nebulous matter cannot be great, because we may there see in one and the same object, both the brightest and faintest appearance of nebulosities that can be seen any where. It will therefore be a case of some interest, if we can form any conception of the place among the fixed stars to which we ought to refer the situation of this nebula; and this I believe my observation of it will enable us to determine pretty nearly.

In the year 1774, the 4th of March, I observed the nebulous star, which is the 43d of the *Connoissance des Temps*, and is not many minutes north of the great nebula; but at the same time I also took notice of two similar, but much smaller nebulous stars; one on each side of the large one, and at nearly equal distances from it. Fig. 37 is a copy of a drawing which was made at the time of observation.

In 1783, I examined the nebulous star, and found it to be faintly surrounded with a circular glory of whitish nebulosity, faintly joining to the great nebula.

About the latter end of the same year I remarked that it was not equally surrounded, but most nebulous towards the south.

In 1784 I began to entertain an opinion that the star was not connected with the nebulosity of the great nebula of Orion, but was one of those which are scattered over that part of the heavens.

In 1801, 1806, and 1810 this opinion was fully confirmed, by the gradual change which happened in the great nebula, to which the nebulosity surrounding this star belongs. For the intensity of the light about the nebulous star had by this time been considerably reduced, by the attenuation or dissipation of the nebulous matter; and it seemed now to be pretty evident that the star is far behind the nebulous matter, and that consequently its light in passing through it is scattered and deflected, so as to produce the appearance of a nebulous star. A similar phenomenon may be seen whenever a planet or a star of the 1st or 2nd magnitude happens to be involved in haziness; for a diffused circular light will then be seen, to which, but in a much inferior degree, that which surrounds this nebulous star bears a great resemblance.

When I reviewed this interesting object in December 1810, I directed my attention particularly to the two small nebulous stars, by the sides of the large one, and found that they were perfectly free from every nebulous appearance; which confirmed not only my former surmise of the great attenuation of the nebulosity, but also proved that their former nebulous appearance had been entirely the effect of the passage of their feeble light through the nebulous matter spread out before them.

The 19th of January 1811, I had another critical examination of the same object in a very clear view through the 40-feet telescope; but notwithstanding the superior light of this instrument, I could not perceive any remains of nebulosity about the two small stars, which were perfectly clear, and in the same situation, where about thirty-seven years before I had seen them involved in nebulosity.

If then the light of these three stars is thus proved to have undergone a visible modification in its passage through the nebulous matter, it follows that its situation among the stars is less distant from us than the largest of the three, which I suppose to be of the 8th or 9th magnitude. The farthest distance therefore, at which we can place the faintest part of the great nebula in Orion, to which the nebulosity surrounding the star belongs, cannot well exceed the region of the stars of the 7th or 8th magnitude, but may be much nearer; perhaps it may not amount to the distance of the stars of the 3d or 2nd order; and consequently the most luminous appearance of this nebula must be supposed to be still nearer to us. From the very considerable changes I have observed in the arrangement of its nebulosity, as well as from its great extent, this inference seems to have the support of observation; for in very distant objects we cannot so easily perceive changes as in near ones, on account of the smaller angles which both the objects and its changes subtend at the eye. The following memorandum was made when I viewed it in 1774; "its "shape is not like that which Dr. SMITH has delineated in his "optics, though somewhat resembling it, being nearly as in "fig. 37: from this we may infer that there are undoubtedly "changes among the regions of the fixed stars; and perhaps

" from a careful observation of this lucid spot, something may " be concluded concerning the nature of it."

In January 1783, the nebulous appearance differed much from what it was in 1780, and in Septemper it had again undergone a change in its shape since January.

March 19, 1811. With a view to ascertain such obvious alterations in the disposition of the nebulous matter as may be depended on, I selected a telescope that had the same light and power which thirty-seven years ago I used, when I made the above-mentioned drawing; and the relative situation of the stars remaining as before, I found that the arrangement of the nebulosity differs considerably. The northern branch *N* still remains nearly parallel to the direction of the stars *a b*; but the southern branch *S* is no longer extended towards the star *d;* its direction is now towards *e*, which is very faintly involved in it. The figure of the branch is also different; the nebulosity in the parallel *P F* of the three stars being more advanced towards the following side than it was formerly.

I compared also the present appearance of this nebula with the delineation which HUYGHENS has given of it in his *Systema Saturnium*, page 8, of which fig. 38 is a copy. The twelve stars which he has marked are sufficient to point out the arrangement of the nebulous matter at the time of his observation. By their situation we find that the nebula had no southern branch, nor indeed any to the north, unless we call the nebulosity in the direction of the parallel a branch; but then this branch is not parallel to a line drawn from *a* to the star *b;* moreover the star *f* is now involved in faint nebulosity, which also reaches nearly up to *g*, and quite incloses *h*. The star *b*

which is now nebulous, is represented as perfectly out of all nebulosity, and can hardly be supposed to have been affected when HUYGHENS observed it.

The changes that are thus proved to have already happened, prepare us for those that may be expected hereafter to take place, by the gradual condensation of the nebulous matter; for had we no where an instance of any alteration in the appearance of nebulæ, they might be looked upon as permanent celestial bodies, and the successive changes, to which by the action of an attracting principle they have been conceived to be subject, might be rejected as being unsupported by observation.

The various appearances of this nebula are so instructive, that I shall apply them to the subject of the partial opacity of the nebulous matter, which has already been inferred from its planetary appearance, when extremely condensed in globular masses; but which now may be supported by more direct arguments. For when I formerly saw three fictitious nebulous stars, it will not be contended that there were three small shining nebulosities, just in the three lines in which I saw them, of which two are now gone and only one remaining. As well might we ascribe the light surrounding a star, which is seen through a mist, to a quality of shining belonging to that particular part of the mist, which by chance happened to be situated where the star is seen. If then the former nebulosity of the two stars which have ceased to be nebulous can only be ascribed to an effect of the transit or penetration of their light through nebulous matter which deflected and scattered it, we have now a direct proof that this matter can exist in a state of

opacity, and may possibly be diffused in many parts of the heavens without our being able to perceive it.

That there has been shining as well as opaque nebulous matter about the large star, appears from several observations I have made upon the light which surrounded it. In 1783 the nebulosity about it was so considerable in brightness, and so much on one side of it, that the star did not appear to have any connection with it. The reason of which is plainly, that the shining quality of the nebulous matter then overpowered the feeble scattering of the light of the star in the nebulosity.

32. *Of Stellar Nebulæ.*

It has been remarked that diffused nebulosities may exist unknown to us, among the more distant regions of the fixed stars; and though we may not be able to see a nebulous diffusion that is farther from us than the moderate distance at which we now have reason to suppose the faintest visible nebulosity of the nebula in Orion to be placed; yet if some former diffusion of the nebulous matter should be already reduced into separate and much condensed nebulæ, they might then come within the reach of telescopes that have a great power of collecting light: this being admitted, there is a probability that some of the various diffusions of the nebulous matter, from which our present nebulæ derive their origin, may have been much farther from us than others. For, in every description of figure, size and condensation, of which I have given instances, we find not only very bright and very large, but also faint and small, as well as extremely faint and extremely small nebulæ; and the same gradations will now be found to run through that class which I have called stellar

nebulæ. This classification was introduced in my sweeps when the objects to be recorded came in so quick a succession that I found it expedient to express as much as I could in as few words as possible, and by calling a nebula stellar, I intended to denote that the object to which I gave this name was, in the first place as small, or almost as small, as a star; and in the next, that notwithstanding its smallness, and starlike appearance, it bore evident marks of not being one of those objects which we call stars, and of which I saw many at the same time in the telescope.

The following three collections contain one hundred and seventeen stellar nebulæ, which have been assorted by their brightness, that their comparative condensation might be estimated according to the different distances at which we may suppose other nebulæ of the same degree of light to be placed.*

I. 71 is "A considerably bright, very small, almost stellar "nebula; the brightness diminishing insensibly and breaking "off pretty abruptly. The whole together is not more than "about 7 or 8″ in diameter." A second observation, made in a "remarkable clear morning, says, that "the greatest bright-"ness is towards the following side, and that the very faint "nebulosity extends to near a minute."

This is probably a condensation of a former nucleus with surrounding chevelure.

I. 268 is "A very bright, very small, round stellar nebula." Fig. 39.

This may be a former planetary nebula in a higher state of condensation.

* See *First assortment containing six of the brightest stellar nebulæ.* I. 71, 268. II. 110, 603. IV. 32, 46.

II. '110 is " A very bright small stellar nebula or star " with a bur all-around." Fig. 40.

This star with a bur is probably one that was formerly a planetary nebula with a pretty strong haziness on the borders.

II. 603 is " A pretty bright stellar nebula, or a pretty considerable star with a very faint chevelure." Fig. 41.

This may have been a planetary nebula with a faint haziness about the margin.

IV. 46 is " A very small pretty bright, or considerably " bright stellar nebula, like a star with burs."

It may have been a pretty well defined planetary nebula.

If it should be deemed singular that we have not a greater number of bright stellar nebulæ, I must remark that, if the stellar is a succession of the planetary state, the number of bright stellar is sufficiently proportionable to that of the planetary nebulæ; and as the faint nebulæ are far more numerous than the bright ones, so it will be seen by the references in the two next assortments, that in proportion as brightness decreases, we have a much more copious collection of stellar nebulæ.*

II. 663 is " A pretty bright very small stellar nebula."

This nebula and the rest of them, which are all of the same description, must be looked upon as condensations of distant nebulæ that had nuclei, or were nearly about the planetary condition.†

* See *Second assortment containing eleven stellar nebulæ of the next degree of brightness.* II. 159, 178, 179, 204, 232, 663, 676, 689, 708, 820, 867.

† See *Third assortment containing one hundred stellar nebulæ of several degrees of faintness.* II. 127, 194, 244, 340, 341, 425, 443, 448, 449, 454, 550, 551, 576, 618, 620, 692, 693, 718, 721, 722, 735, 740, 781, 815, 848. III. 81, 109, 114, 119,

In this collection of nebulæ we have many of a different description. In some, the mark whereby they were distinguished from stars was their figure, the object not being so small but that its figure might still be perceived. Of others, some difference in the brightness between the center and outside was visible; and many of them were only called stellar, because by some deficiency or other in the appearance it was evident they were not perfect stars. Instances of every sort will be seen in the following descriptions.

II. 424 is "A very faint stellar nebula, or a little larger."

II. 805 is "An extremely faint very small round stellar nebula."

II. 425 is "A faint very small stellar nebula, of an irregular figure."

III. 145 is "A very faint stellar nebula; a little extended."

III. 691 is "A considerably faint stellar nebula, suddenly much brighter in the middle."

38. *Of Stellar Nebulæ nearly approaching to the Appearance of Stars.*

The starlike appearance of the following six nebulæ is so considerable that the best description, which at the time of observation I could give of them, was to compare them to stars with certain deficiencies.*

125, 136, 145, 151, 161, 167, 168, 169, 170, 171, 172, 173, 175, 188, 215, 231, 232, 234, 240, 260, 276, 277, 278, 289, 294, 320, 322, 341, 400, 401, 418, 422, 423, 424, 438, 439 469, 476, 530, 536, 561, 562, 563, 564, 565, 571, 576, 590, 606, 627, 672, 691, 706, 737, 741, 764, 768, 770, 772, 777, 786, 793, 805, 815, 821, 828, 843, 852, 855, 856, 916.

* See *Three stars with bars*, II. 655. IV. 47, 49.

IV. 49 is "A pretty bright stellar nebula, like a star with a "small bur all around."

The other two are of the same nature.*

IV. 15 is "A stellar nebula, or rather like a faint star with "a small chevelure and two burs."

The other two are nearly of the same description.

34. *Of doubtful Nebulæ.*

It may have been remarked, that many stellar nebulæ of my catalogues have the memorandum added to their descriptions that they were confirmed with a higher magnifying power, and that this was sometimes attended with difficulty, and sometimes could not be successively done.

A collection of thirty-four nebulæ that come under this description is as follows:†

II. 470 is "A small stellar nebula." By a second observation a doubt entertained in the first was removed with 240, which shewed it "pretty bright, but hardly to be distinguished from a star."

III. 29 is "A very faint extremely small stellar nebula or "rather nebulous star." The sweeping power left me rather "doubtful; 240 verified it.

It must be noticed, that in these nebulæ the doubt which was entertained did not relate to the existence of the objects, but merely to their nature; and when the suspected nebula was so faint that even its existence was doubtful, a higher

* See *Three stars with a faint chevelure.* IV. 15, 21, 31.

† See *First assortment containing twenty-five verified stellar nebulæ.* II. 470, 502, 661. III. 29, 80, 84, 124, 135, 174, 184, 187, 202, 207, 214, 226, 264 266, 268, 269, 513, 549, 604, 742, 748, 964.

power was applied only with a view to ascertain whether the object existed as nebula or as star; for had the suspicion of its existence not been accompanied with the expectation of its being a nebula, it could never have been attempted to be verified.*

III. 270 is "A very faint extremely small stellar nebula; "240 verified it with difficulty, and considerable attention, the "night being uncommonly clear."

When difficulty is mentioned, it is always to be understood that a considerable time as well as attention was required in the examination before a decisive opinion could be formed.†

III. 7 is "A nebulous star, but doubtful of the nebulosity. "With 240 the same doubtful appearance continues." Fig. 42.

With this object the doubt which remained could only relate to the nature of it; for being at first sight taken to be a nebulous star, its existence could not be a subject for examination; but the unresolved doubt, whether an object is a nebula or a star, must certainly be allowed to be as great a proof of identity as we can possibly expect to see.

35. *Concluding Remarks.*

The total dissimilitude between the appearance of a diffusion of the nebulous matter and of a star, is so striking, that an idea of the conversion of the one into the other can hardly occur to any one who has not before him the result of the critical

* See *Second assortment, containing five stellar nebulæ verified with difficulty.* III. 115, 212, 219, 262, 270.

† See *Third assortment, containing four objects that could not be verified.* III. 7, 176, 263, 293.

examination of the nebulous system which has been displayed in this paper. The end I have had in view, by arranging my observations in the order in which they have been placed, has been to shew, that the above mentioned extremes may be connected by such nearly allied intermediate steps, as will make it highly probable that every succeeding state of the nebulous matter is the result of the action of gravitation upon it while in a foregoing one, and by such steps the successive condensation of it has been brought up to the planetary condition. From this the transit to the stellar form, it has been shown, requires but a very small additional compression of the nebulous matter, and several instances have been given which connect the planetary to the stellar appearance.

The faint stellar nebulæ have also been well connected with all sorts of faint nebulæ of a larger size; and in a number of the smaller sort, their approach to the starry appearance is so advanced, that in my observations of many of them it became doubtful whether they were not stars already.

It must have been noticed, that I have confined myself in every one of the preceding articles to a few remarks upon the appearance of the nebulous matter in the state in which my observations represented it; they seemed to be the natural result of the observations under consideration, and were not given with a view to establish a systematic opinion, such as will admit of complete demonstration. The observations themselves are arranged so conveniently that any astronomer, chemist, or philosopher, after having considered my critical remarks, may form what judgment appears most probable to him. At all events, the subject is of such a nature as cannot fail to attract the notice of every inquisitive mind to a contem-

plation of the stupendous construction of the heavens; and what I have said may at least serve to throw some new light upon the organization of the celestial bodies.

Synopsis of the Contents of this Paper.

Diffused nebulosity exists in great abundance, - page 272
Observations of more than one hundred and fifty square degrees of it, - - - - - - 275
Its abundance exceeds all imagination, - - 277
Nebulous matter consists of substances that give out light, which may have many other properties, - 277
Nebulous diffusions contain both milky nebulosity, and such as from its faint appearance may be mistaken for resolvable, - - - 278, 281
The range of its visibility is confined to very moderate limits, - - - - - 278, 279
Much of it may be at distances where it cannot be seen, 280
The purpose for which such great abundance of nebulosity may exist, deserves investigation, - - 280
Either greater depth or greater compression of the nebulous matter may occasion greater brightness, 282
Condensation will best account for greater brightness, 284
The condensation of the nebulous matter ascribed to gravitation, - - - - - 284
When a nebula has more than one preponderating seat of attracting matter, it will probably in time be divided, 285

This conception is supported by the appearance of double nebulæ, - - - - page 285,
Their double appearance can be no deception, - 286
Their nebulosity is derived from one common stock, 286
This opinion is supported by the remarkable situation of nebulæ, - - - - 290
Which may be seen in Mr. Bode's Atlas Cœlestis, - 292
The real form of the nebulous matter of which nebulæ are composed may be inferred from their figure, 293, 294, 295, 296.
The form of the nebulous matter of round nebulæ is globular, - - - - - 299
This form is caused by gravitation, - - 299
The central brightness of nebulæ points out the seat of attraction, - - - - - 302
The effect of attraction on the form of the nebulous matter depends on its original expansion, or the time of the action and on the quantity of the attracting matter, 302
Three different stages of condensation pointed out, 305
Comets may be highly condensed nebulæ, - - 306
Progressive condensation takes place, - - 307
Concentric brightness as well as globular form indicates the general gravitation of the nebulous matter, 308
Progressive condensation may be seen in the formation of nuclei, - - - - - 308
Nebulous matter is probably capable of being consolidated; the act of shining proves it to have chemical properties, - - - - - 309, 310
It will stop light, and is partly opaque, - - 310
Queries relating to the subsidence of the nebulous matter, the zodiacal light, and the cause of rotatory motion, 311

Some part of the nebulous matter is probably elastic, 313
The uniform light of nuclei, and of much condensed nebulæ, proves that the nebulous matter is considerably opaque, - - - - - 313
When the nebulous matter assumes hardness, the progress of condensation will be impeded, - 314
Three indications of the compression of the nebulous matter, - - - - - 315
Planetary appearance arises from superficial lustre, 315
High degree of condensation of the nebulous matter, 316
A still higher degree of condensation, - - 316
In common good telescopes planetary nebulæ cannot be distinguished from stars, - - - 318
Perhaps they may in the end be so condensed as actually to become stars, - - - - 318
The nebulous matter in a cubical space of 10′ will admit of a condensation of two trillion and 208 thousand billion times before it can go into the compass of a globe of the diameter of our sun, - - 319
Planetary nebulæ have a rotatory motion on their axes, 319
The original eccentricity of the nebulous matter of a nebula may be the physical cause of the rotatory motion of celestial bodies, - - - 319
The nebulous star in Orion is fictitious, - 320
Two out of three nebulous stars in Orion have lost their former neb lous appearance, - - 321
Their fictitious appearance was owing to a dispersion of their light in passing through nebulous matter, 321
The faintest appearance of the nebulosity in Orion is perhaps not further from us than the stars of the 3d

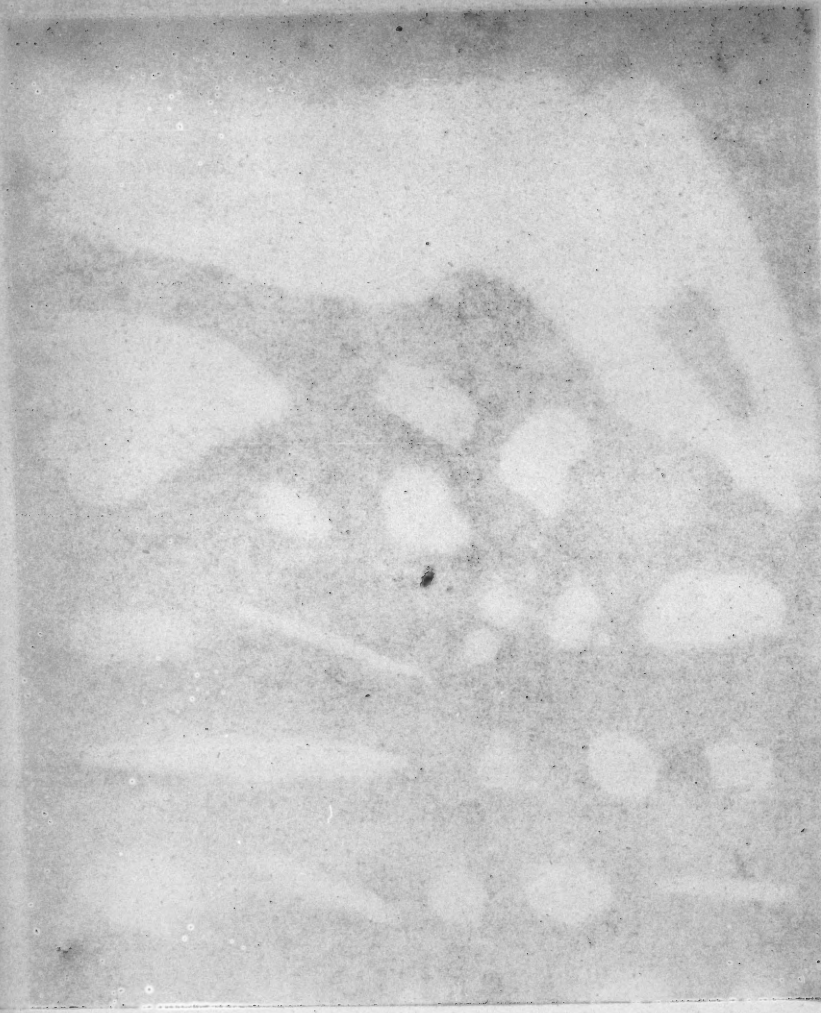
or 2nd magnitude; the brightest part is therefore probably not so far, - - - 322
In thirty-seven years the nebulosity of this nebula has undergone great changes and much greater since the time of HUYGHENS, - - - 323
Nebulæ are not permanent celestial bodies, - 324
Additional proof of the opacity of the nebulous matter, 325
Very distant nebulosities, which cannot be seen in a state of diffusion, may become visible when condensed into separate nebulæ, - - - - 325
Conversion of planetary into bright stellar nebulæ, into stars with burs, or stars with haziness, - - 327
Conversion of more distant ones into faint stellar nebulæ; into stars with burs, or with faint chevelure, - 329
When it is doubtful whether an object is a star or a nebula, it may be verified by an increase of magnifying power, - - - - - 329
When the object is very like a star, it becomes difficult to ascertain whether it is a star or a nebula, - 330
When we cannot ascertain whether the doubtful object is a star or a nebula, of which several instances are given, the similitude is as great as any we can expect; for were it greater, there could be no doubt, - 330

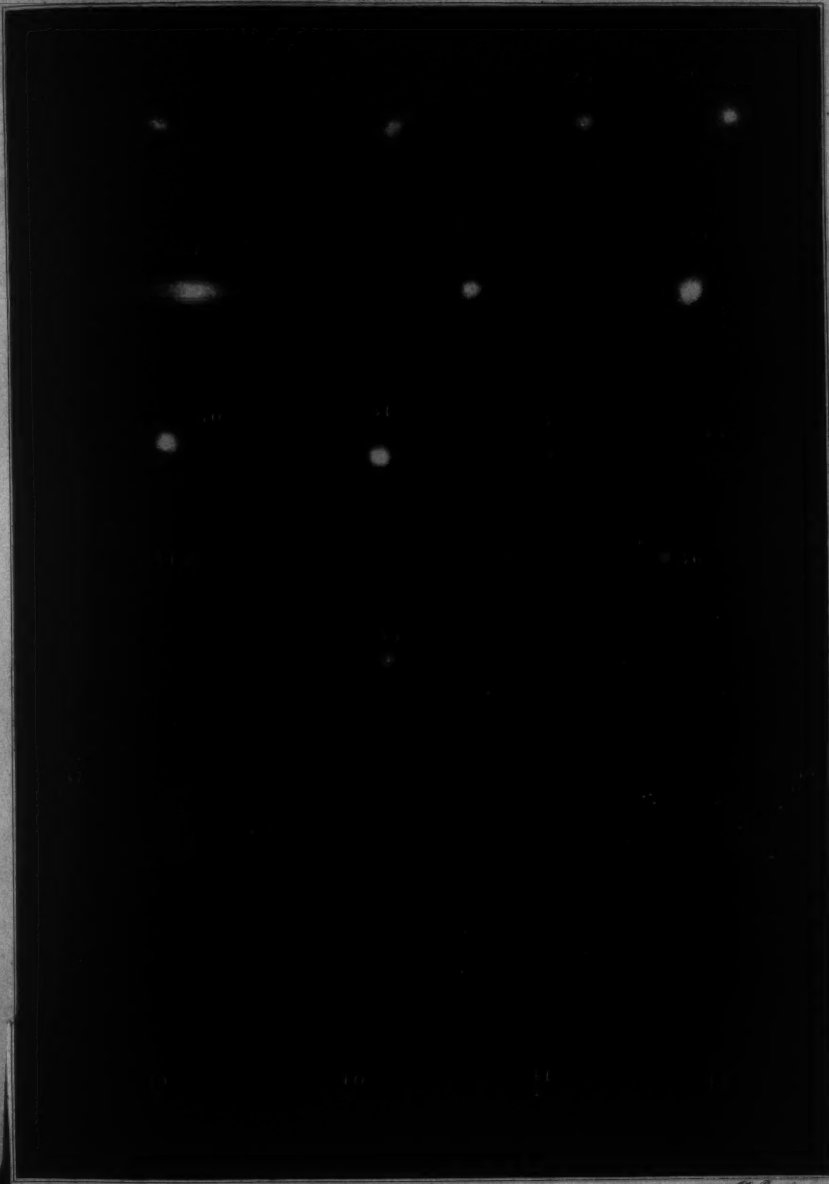
POSTSCRIPT.

IT will be seen that in this paper I have only considered the nebulous part of the construction of the heavens, and have taken a star for the limit of my researches. The rich collection of clusters of stars contained in the 6th, 7th, and 8th classes of my Catalogues, and many of the *Connoissance des Temps*, have as yet been left unnoticed. Several other objects, in which stars and nebulosity are mixed, such as nebulous stars, nebulæ containing stars, or suspected clusters of stars which yet may be nebulæ, have not been introduced, as they appeared to belong to the sidereal part of the construction of the heavens, into a critical examination of which it was not my intention to enter in this Paper.

WILLIAM HERSCHEL.

Slough, near Windsor,
May 26, 1811.





XVII. *Experiments to ascertain the State in which Spirit exists in fermented Liquors: with a Table exhibiting the relative Proportion of pure Alcohol contained in several Kinds of Wine and some other Liquors.* *By* William Thomas Brande, *Esq. F. R. S.*

Read June 13, 1811.

SECTION I.

It has been a commonly received opinion, that the alcohol obtained by the distillation of wine, does not exist ready formed in the liquor, but that it is principally a product of the operation, arising out of a new arrangement of its ultimate elements.

The proofs which have been brought forward in support of this theory, are chiefly founded on the researches of Fabroni,* who attempted to separate alcohol by saturating the wine with dry subcarbonate of potash, but did not succeed, although by the same means he could detect very minute portions of alcohol which had been purposely added.

To obtain satisfactory results from many of the following experiments, it became necessary to employ wines to which little or no spirit had been added; for a very considerable addition of brandy is made to most of the common wines, even before they are imported into this country. I therefore occasionally used Burgundy, Hermitage, Cote Roti, Champagne, Frontignac,

* Annales de Chimie, XXXI. p. 303.

and some other French wines, to which, when of the best quality, no spirit can be added, as even the smallest proportion impairs the delicacy of their flavour, and is consequently readily detected by those who are accustomed to taste them. For these, and for the opportunity of examining many of the scarce wines enumerated in the table annexed to this Paper, I am indebted to the liberality of the Right Hon. Sir JOSEPH BANKS.

Dr. BAILLIE, who took considerable interest in this investigation, was also kind enough to procure for me some port wine, sent from Portugal for the express purpose of ascertaining how long it would remain sound, without any addition whatever of spirit having been made to it.

Lastly, I employed raisin wine, which had been fermented without the addition of spirit.

At a very early period of the present inquiry, I ascertained by the following experiments, that the separation of the alcohol, by means of subcarbonate of potash, was interfered with, and often wholly prevented by some of the other ingredients of the wine.

A pint of port wine was put into a retort placed in a sand heat, and eight fluid ounces were distilled over, which by saturation with dry subcarbonate of potash, afforded about three fluid ounces of tolerably pure spirit floating on the surface.

I repeated this distillation precisely under the same circumstances, and mixed the distilled liquor with the residuum in the retort, conceiving that if the spirit were a product, I now should have no difficulty in separating it from the wine by the addition of subcarbonate of potash; but although every pre-

caution was taken, no spirit separated: a portion of the subcarbonate, in combination with some of the ingredients of the wine, formed a gelatinous compound, and thus prevented the appearance of the alcohol.

It has been remarked by FABRONI, in the Memoir above quoted, that one hundredth part of alcohol purposely added to wine may be separated by subcarbonate of potash, but several repetitions of the experiment have not enabled me to verify this result; when however a considerable addition of alcohol has been made to the wine, a part of it may be again obtained by saturation with the subcarbonate. The necessary addition of spirit to port wine, for this purpose, will be seen by the following experiments.

Four ounces of dry and warm subcarbonate of potash were added to eight fluid ounces of port wine, which was previously ascertained to afford by distillization 20 per cent. of alcohol (by measure), of the specific gravity of 0,825 at 60°.

In twenty-four hours the mixture had separated into two distinct portions; at the bottom of the vessel was a strong solution of the subcarbonate, upon which floated a gelatinous substance, of such consistency as to prevent the escape of the liquor beneath when the vessel was inverted, and which appeared to contain the alcohol of the wine, with the principal part of the extract, tan, and colouring matter, some of the subcarbonate, and a portion of water; but as these experiments relate chiefly to the spirit contained in wine, the other ingredients were not minutely examined.

To seven fluid ounces of the same wine, I added one fluid ounce of alcohol (specific gravity 0,825), and the same quantity of the subcarbonate of potash as in the last experiment;

but after twenty-four hours had elapsed, no distinct separation of alcohol had taken place.

When two fluid ounces of alcohol were added to six fluid ounces of the wine, and the mixture allowed to remain undisturbed for the same length of time as in the former experiments, a stratum of impure alcohol, of about a quarter of an inch in thickness, separated on the surface.

The addition of three fluid ounces of the alcohol to five fluid ounces of the wine, formed a mixture from which a quantity of spirit readily separated on the surface, when the subcarbonate was added, and the gelatinous compound sunk nearly to the bottom of the vessel, there being below it a strong solution of the subcarbonate.

When in these experiments Madeira and Sherry were employed instead of Port wine, the results were nearly similar.

It was suggested to me by Dr. WOLLASTON, that if the wine were previously deprived of its acid, the subsequent separation of the alcohol, by means of potash, might be less interfered with. I therefore added, to eight fluid ounces of port wine, a sufficient quantity of carbonate of lime to saturate the acid, and separated the insoluble compounds produced by means of a filter. The addition of potash rendered the filtered liquor turbid, some soluble salt of lime, probably the malate, having passed through the paper; but the separation of alcohol was as indistinct, as in the experiments just related.

It is commonly stated, that the addition of lime water to wine, not only forms insoluble compounds with the acids, but also with the colouring matter, and that these ingredients may be thus separated without heat; but on repeating these experiments, they did not succeed, nor could I devise any

mode of perfectly separating the acids, and the extractive and colouring matter (excepting by distillation), which did not interfere with the alcohol.

If the spirit afforded by the distillation of wine were a *product* and not an *educt*, I conceived that by performing the distillation at different temperatures, different proportions of spirit should be obtained.

The following are the experiments made to ascertain this point.

Four ounces of dried muriate of lime were dissolved in eight fluid ounces of the Port wine employed in the former experiments: by this addition, the boiling point of the wine, which was 190° FAHRENHEIT, was raised to 200°. The solution was put into a retort placed in a sand heat, and was kept boiling until four fluid ounces had passed over into the receiver, the specific gravity of which was 0,96316 at 60° FAHRENHEIT.*

The experiment was repeated with eight fluid ounces of the wine without any addition, and the same quantity was distilled over, as in the last experiment: its specific gravity at 60° FAHRENHEIT, was 0,96311.

Eight fluid ounces of the wine were distilled in a water bath; when four fluid ounces had passed over, the heat was withdrawn. The specific gravity of the liquor in the receiver was 0,96320 at 60° FAHRENHEIT.

The same quantity of the wine, as in the last experiment, was distilled at a temperature not exceeding 180° FAHRENHEIT. This temperature was kept up from four to five hours, for

* It was supposed that in this experiment a small portion of muriate of lime might have passed over into the receiver, but the distilled liquor did not afford the slightest traces of it, to the tests of oxalate of ammonia and nitrate of silver.

five successive days, at the end of which period, four ounces having passed into the receiver; its specific gravity at 60° was ascertained to be 0,96814.

It may be concluded, from these results, that the proportion of alcohol is not influenced by the temperature at which wine is distilled, the variation of the specific gravities in the above experiments being even less than might have been expected, when the delicacy of the operation by which they are ascertained, is considered.

I have repeatedly endeavoured to separate the spirit from wine, by subjecting it to low temperatures, with a view to freeze the aqueous part; but when the temperature is sufficiently reduced, the whole of the wine forms a spongy cake of ice.

In a mixture of one fluid ounce of alcohol with three of water, I dissolved the residuary matter, afforded by evaporating four fluid ounces of Port wine, and attempted to separate the alcohol from this artificial mixture by freezing; but a spongy cake of ice was produced as in the last experiment.

When the temperature is more gradually reduced, and when large quantities of wine are operated upon, the separation of alcohol succeeds to a certain extent, and the portion which first freezes is principally, if not entirely water, hence in some countries this method is employed to render wine strong.

SECTION II.

Having ascertained that alcohol exists in wine ready formed, and that it is not produced during distillation, I employed that process to discover the relative proportion of alcohol contained in different wines.

In the following experiments, the wine was distilled in glass retorts, and the escape of any uncondensed vapour was prevented by employing sufficiently capacious receivers, well luted, and kept cold during the experiment.

By a proper management of the heat towards the end of the process, I could distil over nearly the whole of the wine without burning the residuary matter: thus, from a pint of Port wine, of Madeira, of Sherry, &c. I distilled off from fifteen fluid ounces, to fifteen fluid ounces and a half; and from the same quantity of Malaga, and other wines containing much saccharine matter, I could readily distil from fourteen to fifteen fluid ounces.

In order to ascertain the proportion of alcohol with precision, pure water was added to the distilled wine, so as nearly to make up the original measure of the wine, a very small allowance being made for the space occupied by the solid ingredients of the wine, and for the inevitable loss during the experiments: thus, five fluid drachms and a half of distilled water were added to fifteen fluid ounces and a quarter of the liquor procured by the distillation of a pint of port wine, and in other cases nearly the same proportions were observed. This mixture of the distilled wine and water, was immediately transferred into a well stopped phial, and having been thoroughly agitated, was allowed to remain at rest for some

hours; its specific gravity (at the temperature of 60° FAHRENHEIT), was then very carefully ascertained, by weighing it in a bottle holding exactly one thousand grains of distilled water at the above temperature, and the proportion of alcohol per cent. *by measure*, was estimated by a reference to Mr. GILPIN's tables,* the specific gravity of the standard alcohol being 0,82500 at 60°.

As the most convenient mode of exhibiting the results of these numerous experiments, I have thrown them into the form of a table; in the first column the wine is specified; the second contains its specific gravity after distillation, as above described; and the third exhibits the proportion of the pure spirit, which every hundred parts of the wine contain. I have also inserted porter, ale, cyder,† brandy, and some other spirituous liquors, for the convenience of comparing their strength, with that of the wines.

* Phil. Trans. 1794.

† The proportion of spirit, which may be obtained from these three liquors, is subject to considerable variation in different samples: the number given for each, in the table, is therefore the mean of several experiments, as it did not seem necessary to specify them separately.

Wine.	Specific Gravity after Distillation.	Proportion of Alcohol, per Cent. by Measure.
Port - -	0,97616	21,40
Ditto -	0,97532	22,30
Ditto - -	0,97430	23,39
Ditto -	0,97400	23,71
Ditto - -	0,97346	24,29
Ditto -	0,97200	25,83
Madeira - -	0,97810	19,34
Ditto - -	0,97616	21,40
Ditto - -	0,97380	23,93
Ditto -	0,97333	24,42
Sherry - -	0,97913	18,25
Ditto - -	0,97862	18,79
Ditto -	0,97765	19,81
Ditto - -	0,97700	19,83
Claret -	0,98440	12,91
Ditto - -	0,98320	14,08
Ditto -	0,98092	16,32
Calcavella -	0,97920	18,10
Lisbon - -	0,97846	18,94
Malaga -	0,98000	17,26
Bucellas - -	0,97890	18,49
Red Madeira -	0,97899	18,40
Malmsey Madeira	0,98090	16,40
Marsala - -	0,97196	25,87
Ditto - -	0,98000	17,26
Red Champagne -	0,98608	11,30
White Champagne	0,98450	12,80
Burgundy -	0,98300	14,53
Ditto - -	0,98540	11,95
White Hermitage	0,97990	17,48
Red Hermitage -	0,98495	12,32
Hock - -	0,98290	14,37
Ditto - -	0,98873	8,88
Vin de Grave -	0,98450	12,80

Wine.	Specific Gravity after Distillation.	Proportion of Alcohol, per Cent. by Measure.
Frontignac - -	0,98452	12,79
Cote Roti -	0,98495	12,32
Rousillon - -	0,98005	17,26
Cape Madeira -	0,97924	18,11
Cape Muschat -	0,97913	18,25
Constantia -	0,97770	19,75
Tent - -	0,98399	13,30
Sheraaz -	0,98176	15,59
Syracuse - -	0,98200	15,28
Nice - -	0,98263	14,63
Tokay - -	0,98760	9,88
Raisin Wine -	0,97205	25,77
Grape Wine -	0,97925	18,11
Currant Wine -	0,97696	20,55
Gooseberry Wine	0,98550	11,84
Elder Wine -	0,98760	9,87
Cyder - -	0,98760	9,87
Perry - -	0,98760	9,87
Brown Stout -	0,99116	6,80
Ale - -	0,98873	8,88
Brandy -	0,93544	53,39
Rum - -	0,93494	53,68
Hollands -	0,93855	51,60

XVIII. *Account of a Lithological Survey of Schehallien, made in order to determine the specific Gravity of the Rocks which compose that Mountain.* By John Playfair, *Esq. F. R. S.*

Read June 27, 1811.

THE astronomical observations made on the mountain Schehallien, in 1774, were confessedly of great importance to science. They ascertained the power of mountains to produce a sensible disturbance in the direction of the plumb-line; of consequence, they proved the general diffusion of gravity through terrestrial substances, and afforded data for determining the medium density of the earth, compared with that of the bodies at its surface.

The skill with which this very delicate experiment was conducted by Dr. MASKELYNE, and the ingenuity with which the results were deduced by Dr. HUTTON, were worthy of the objects in view, and of the reputation which these distinguished men have acquired in their respective departments of the mathematical sciences.

One thing only seemed wanting to give to the determination of the earth's density all the accuracy that could be obtained from a single experiment, namely, a more precise knowledge of the specific gravity of the rock which composes the mountain, as being the object with which the mean density of the earth was immediately compared. The specific gravity of that rock was assumed to be to that of water as 5 to 2;

which, though it be nearly a medium when stones of every kind, from the lightest to the heaviest, are included, is certainly too small for Schehallien, the rocks of which belong to a class of a specific gravity considerably above the mean. The uncertainty arising from this source might not be of great amount, yet it was desirable that the quantity, or, at least, the limits of it should be accurately ascertained. In this light I knew, from repeated conversations, that the matter was regarded by both the gentlemen above named.

I had therefore long wished to attempt such a survey of the mountain as might afford a satisfactory solution of this difficulty; and having mentioned the circumstances to the Right Hon. Lord WEBB SEYMOUR, he entered readily into a scheme, which without the assistance of his skill and activity, I should have been quite unable to carry into execution.

Accordingly, in June 1801, we took up our residence in a small village as near as we could to the bottom of the mountain, and began our Mineralogical Survey, the result of which we think it our duty to submit to the Society, under the auspices of which the original experiment was undertaken.

It was obvious, that our first object must be to obtain specimens of all the varieties of rock in the mountain, which had any considerable difference in their external characters. These specimens must be such as had not been exposed to the action of the weather, were perfectly sound, with a fresh fracture, and taken from the living rock. In order to procure these, we soon found that it was not necessary to dig into the mountain or to blast the stones with gun-powder, for the native rock breaks out on the bare and rugged surface in abundance of places, and is so deeply intersected by the

streams that it was easy, by the assistance of the hammer only, to procure specimens having all the conditions requisite for our purpose.

Supposing, however, that all this was accomplished, it would be insufficient to determine the mean density of the mountain, unless the quantity of rock of each particular kind could also be estimated; at least nearly. It was necessary, therefore, to know what proportion of the mountain consisted of one species of rock, and what of another, without which the average could not be determined.

Had the mean density been the only thing wanted, it would have been sufficient to know the quantity of each variety of rock; but in the search we were engaged in, it was necessary to know not only the quantity, but the position of each of these varieties, relatively to the observatories on the south and north faces of the mountain. This will be evident, when it is considered that it was the effect of each portion of the rock on the plumb-line in these observatories that was the thing to be found, and that this effect must vary not only with the density of the rock, but with its distance from the observatory, and its obliquity in respect of the meridian. The mean density would therefore be insufficient for estimating the attraction of the mountain, could it be found ever so exactly; and it is easy to shew, that while the mean density of a heterogeneous mass, and also its magnitude and figure remain the same, its attractive force at a given point may be greatly changed by a different distribution of the materials it consists of, relatively to that point. In order then to form an estimate of this attraction we must know, at least nearly, these three things, the varieties of rock composing the mountain; the quantity

of each variety; and, lastly, the position of each relatively to the observatory. Fortunately the Geometrical Survey of the mountain, which had already determined not merely its superficial extent but its solidity, taken in combination, with some peculiarities in its structure, have enabled us to approximate, I hope with some tolerable exactness, to the knowledge of all these three circumstances.

The plan, then, which we proposed to follow, and which was necessary to be pursued if our Lithological Survey was to correspond in any degree to the accuracy of the Geometrical Survey, made under the direction of the Astronomer Royal, was to try to recognise the chain of stations which had been employed in that survey, in order that, by reference to those stations, we might be able to determine the points on the surface of the mountain from which our different specimens were collected. After these stations were discovered, we meant to traverse the mountain in various directions, and at any point where a specimen was taken, to determine our position by the bearings of any two of the stations that might be in sight, or by taking angles to three of them, or such other methods as occasion and circumstances might suggest. This was to be done where considerable variations in the external characters of the rocks gave reason to look for considerable variations of specific gravity. It was an operation that could not be necessary for every individual specimen, but it was one which must be necessary for determining the district over which stone of a particular character prevailed. In this part of the work we were to employ a theodolite, a sextant or a compass, according as more or less accuracy seemed requisite.

As the marks of the stations were all effaced except some

traces of the observatories (or rather the huts in which Dr. MASKELYNE had lived), and the two cairns on the top of the mountain, the discovery of the whole chain was a matter of some difficulty. By means, however, of the bearings, as given in Dr. HUTTON's paper, and the assistance of one of the guides who had been employed about the survey, we succeeded in finding out the stations; and as they were mostly on elevated points, we could distinguish them at a distance with sufficient exactness.

Schehallien belongs to one of the central ridges of the Grampians, which, stretching here from about SE. to NW. divides the vallies of the Tummel and the Tay. Though it be a part of this chain, it stands considerably separate from the rest on a base of a form somewhat oval, and having its figure distinctly defined by two streams that run, the one on the south, and the other on the north side of it. The lowest point in this base, which is on the NE. is 2467 feet below the summit of the mountain, and about 1094 above the level of the sea.

At the NW. extremity, Schehallien adheres to the main chain by means of a high ridge, depressed at its lowest point little more than 1500 feet under the summit. On the opposite sides of this neck the streams rise, which were before said to determine the base of the mountain; these streams, however, do not unite at the eastern extremity of the base, for there also a sort of neck, though very low in comparison of the former, connects Schehallien with the hills to the eastward.

Beyond the streams just mentioned, a range of inferior hills, some of them very low, springing from the main ridge

on the NW. encompasses the mountain, forming as it were a line of circumvallation round it, and on these were the stations which Mr. BURROWS, under Dr. MASKELYNE's direction, had chosen for the survey. Beyond these hills the ground falls down into a sort of plain of great extent on the north; on the south, less considerable and more uneven, yet such as to leave Schehallien very free and open in the direction of the meridian, and adapted by that means to shew the full amount of its action on the plummet. From the base its sides rise with a rapid, though unequal acclivity, and terminate not in a point, but in a ridge or narrow plane of a waving form, about a mile in length, and sloping regularly to the east, where it is 480 feet lower than at the western extremity. Though the sides are very rugged, they are less broken by deep ravines or bold projections than the other mountains of the same elevation in this quarter of the Grampians; for, beside the high neck which has been already mentioned as uniting Schehallien to the mountains on the west it has only one other saliant ridge, which runs out to the NE. and overlooks the plain with a very steep and precipitous aspect. In some directions, and when viewed from a considerable distance, the harsh features of the mountain are wonderfully softened; it acquires a very beautiful conoidal shape, and from thence derives the name by which it is known among the inhabitants of the low country.

The rock of Schehallien, like that of all the mountains in its vicinity, is of the class called primitive; and is disposed for most part in great parallel plates, or strata, nearly vertical, stretching from SE. to NW. They are indeed so nearly vertical, that a deviation of 15° from the perpendicular is rarely

to be met with, except toward the base of the mountain, where it is sometimes greater, and is subject to considerable inequalities. The strata on the north side of the mountain lean a little toward the north, and those on the south toward the south. All these variations, however, are inconsiderable, and in general the strata may be set down as nearly vertical.

But though in their disposition all the rocks of Schehallien agree pretty nearly, they differ considerably in their mineralogical ch. acters. A large proportion of the mountain, and that which constitutes the most elevated part, is formed of a granular quartz, extremely hard, compact, and homogeneous. The whole mass from about the level of the two observatories up to the summit of the mountain, is of this stone. Lower down, again, on every side, the rock is a schistus containing much mica and hornblend; and the division into parallel and vertical plates is more obvious than in the granular quartz. This last, however, is sometimes found in the lower parts, forming thin, vertical plates, interstratified with the hornblend and mica slate, and all together preserving their parallelism with a neatness and accuracy which a work of art could hardly exceed. This is particularly to be observed in the bed of the *burn* of Glenmore, the stream that defines the base of the mountain on the south, and which toward the lower part of its course intersects the strata to a great depth.

Besides these two kinds of rock, we meet in several places toward the base of the mountain with a granular and micaceous limestone highly cristallized, which in one or two places ascends to a considerable height. All these rocks are disposed in strata; but there are also veins or dykes of porphyry and greenstein, which traverse the mountain in different directions;

one of the former kind, of great breadth, cuts it right across from NE. to SW. not far from the point of its greatest elevation. There is no where any appearance of metallic veins.

The quartzy rock of Schehallien is extremely hard and homogeneous, and by its slow and uniform decay, has no doubt given rise to that massive shape, and comparatively unbroken surface, which have been already remarked. Yet even here the work of time is abundantly evident; for the rock being much cut by fissures transverse to its stratification, it separates and falls down in large prismatic fragments. Some of these are of a vast size, and being extremely durable, the accumulation of them where the ground is not too steep to permit them to lie, is very great, so that large tracts of the sides of the mountain are covered with cubical blocks of granular quartz, resting on one another, and *steadied* only by their own weight.

It is remarkable of this quartzy stone, that when exposed for some time to the weather, it acquires the lustre and appearance of white enamel, so that the old weather beaten surface is more clear and shining than that which is immediately produced from a fresh fracture. The reason seems to be, that the stone does not consist of pure quartz, but along with the grains of quartz has a great number of grains of felspar interspersed, which when it is first broken give it an opaque and earthy appearance. These are soon dissolved by the action of the weather; and there is then left over the surface a coat of pure quartz, which has the semi-transparency and vitreous gloss belonging to enamel.

The felspar which enters into the composition of the rock here described, is not always in grains, but in some specimens

is found regularly crystallized. The crystals however are small and very thinly disseminated; were they in more abundance this stone might be accounted a granite, as Professor JAMESON has remarked of a stone of the same kind which he found in the island of Jura.

From the vertical position of the strata we may infer with some probability, that the rock which breaks out any where at the surface, continues the same through the interior of the mountain, in the direction of a perpendicular plane, down to its base, or perhaps to an indefinite depth. The same stratum usually remains of the same nature to a great extent, whenever we have an opportunity of examining it, whether in a horizontal or a perpendicular direction; and it is not to be doubted that the same holds when no such opportunity occurs. When, therefore, we have on the surface a bed of mica slate, or of granular limestone, or of granular quartz, the probability is that the whole stratum all through the mountain is composed of the same materials. I must however confess, that I do not think that this probability is as strong with respect to granular quartz, as it is with respect to the micaceous rocks. These last compose the great mass of the Grampians; and their characters, though not every where the same, change very slowly, and pass from one to another by imperceptible gradations. To the granular quartz this rule does not equally apply; it is not general among the mountains of this tract; it sometimes breaks off suddenly, and is replaced by rocks of a very different nature. We cannot therefore with the same confidence assume the existence of this rock in intermediate points, when we only see it in the extremes. This much, however, we know with certainty, that the whole of the upper part of the

mountain, from about the level of the south observatory to the summit, consists of granular quartz, as no other stone is to be met with any where in that tract. This is the part above O, in the section of the mountain; and the only question is, whether we shall consider the part in the interior of the mountain, immediately under this mass, as consisting of the same rock. When we first examined Schehallien, Lord. WEBB SEYMOUR and I were both of opinion that this was the most probable supposition. Since that time, however, having had an opportunity of examining some other of the Grampians where granular quartz is found at the summit, and where, nevertheless, it is certain that the same rock does not go down into the interior, there has appeared some reason to suspect that this may be true of Schehallien. As the result of the calculus with regard to the earth's density is materially affected by these suppositions, I have given the result as I had first deduced it on the hypothesis, that the interior of the mountain is of granular quartz; and also on the hypothesis that the quartz is confined to the upper part; and that the lower part is entirely composed of mica and hornblend slate.

In the computation which Dr. HUTTON made of the attraction of Schehallien, he supposed its mass divided into 960 vertical columns, and he computed the force with which each of these columns disturbed the direction of a plummet suspended in either observatory, supposing them all homogeneous and two and a half times as dense as water.* Now knowing from our survey, and the combination of geometrical with mineralogical observations, the specific gravity of each of these columns at the surface, and conceiving (what we have shown, with one

* Phil. Trans. Vol. LXVIII. (1778), p. 689, &c.

exception, to be probable) that the column remains the same through its whole length, we can compare the real attraction with that assigned to it in Dr. HUTTON's calculation. The attraction of any column computed on his hypothesis being divided by 2.5, and multiplied by the true specific gravity, will give the real attraction, or effect in disturbing the plumb-line. It is on this principle that our correction is formed, though simplifications occurred that very much diminished the labour of the computation, the nature of the rock leading us in the end to distinguish only two differences of specific gravity; and the ingenious deductions of Dr. HUTTON, together with the excellent order that prevails in his computation, having made it easy to follow a route which he had cleared of all its greatest difficulties.

However, as it was impossible to determine before hand how much the specific gravity of these rocks might differ, it was necessary to conduct the survey so that every individual column, had it been necessary, might have had its specific gravity defined. For this purpose the mountain was traversed in various directions, and the points at which a transition was made from rocks of one character to those of another were carefully noted, and their position ascertained. In selecting the specimens which were to represent the rocks of the several districts into which the mountain thus became divided, attention was paid both to the prevailing stone, and to that which was least common, in order that we might, if possible, get possession both of the mean and the extremes. This was in general the principle that guided our choice of specimens, but the application of it in detail to particular instances does not admit of being explained. The reasons in every such

case that determined us to take one stone and reject another, could only be perceived by an observer on the spot, whose eye was accustomed to judge of the varieties, the plenty or the rarity of the minerals that passed in review before him, by indications which it is impossible to describe in words. We are here therefore with reluctance compelled to request from the reader more credit than we are able to prove to him that we deserve. We know that in doing this we are craving an indulgence which no wise and candid observer ever wished to possess; we sincerely regret that the nature of the subject forces us to make this demand, and that the part of our work which it was most difficult to perform to our own satisfaction, is quite incapable of being explained to the satisfaction of others.

Catalogue of Specimens from Schehallien.

The rocks of the mountain may be divided, as already remarked, into three classes; granular quartz; mica, and hornblend slate; granular limestone. The specific gravities were ascertained by the late Dr. KENNEDY, and it is therefore unnecessary to add that their accuracy may be perfectly relied on. The pieces weighed were between 1000 and 4000 grains: most commonly between 2000 and 3000. Different pieces of the same specimen were often examined. The water used was distilled, and always of a temperature between 60 and 61 degrees.

Quartz.

1. Gray sandstone containing mica in thin layers. Specific gravity = 2.6435.

2. White quartz, very pure. Fracture vitreous. Occurs in

beds chiefly on the NE. side of the mountain. Specific gravity = 2.6437.

3. Quartzy sandstone of a whitish gray colour with thin layers of mica. Specific gravity = 2.6296.

4. Quartzy sandstone. White colour with layers of mica. Much indurated. Somewhat ferruginous. Specific gravity = 2.65367.

5. Indurated sandstone with spiculæ of mica interspersed. Specific gravity = 2.6460.

6. Sandstone much indurated, vitreous shine, interspersed with mica. Specific gravity = 2.6269.

7. Granular quartz from near the summit; contains grains of felspar. Specific gravity = 2.6274.

8. Granular quartz; nearly the same with the preceding. Specific gravity = 2.6109.

9. Sandstone fine grained, slightly marked with iron veins. Specific gravity = 2.6296.

10. Sandstone fine grained, more indurated than the preceding. Specific gravity = 2.6576.

11. Sandstone containing calcareous matter. Specific gravity = 2.6656.

12. Granular quartz, very compact and indurated, but of a stratified structure; a little mica in thin plates. From a mean of several. Specific gravity = 2.6452.

13. Granular quartz of a flesh colour; imperfect crystals of felspar thinly disseminated. This specimen from near the top. From a mean of several peices. Specific gravity = 2.6387.

The mean of these thirteen specimens gives 2.6398 for the upper or quartzy part of the mountain.

Mica and Hornblend Slate.

1. Hornblend slate very compact. Specific gravity = 3.0642.

2. Micaceous schist, with hornblend and a small mixture of quartz. Specific gravity = 2.9385.

3. Black micaceous schistus, fine grained containing hornblend. Specific gravity = 3.0476.

4. Micaceous schistus containing pyrites and quartz in fine grains. Specific gravity = 2.7293.

5. Micaceous schistus tinged with an oxid of iron. Specific gravity = 2.7935.

6. Micaceous schist with thin plates of mica and hornblend transverse to the stratification. Specific gravity = 2.7907.

7. Micaceous schist with quartz in small grains. Specific gravity = 2.7499.

8. Another specimen nearly the same. Specific gravity = 2.7728.

9. Compact micaceous schistus, grains of felspar and quartz intermixed. Specific gravity = 2.71845.

10. Nearly the same with the preceding. Specific ra vity = 2.7206.

The medium specific gravity of these ten specimens is 2.83255.

Limestone.

1. Granular limestone of a gray colour, containing some mica. Specific gravity = 2.7087.

2. Granular limestone, silver coloured, stratified structure. Specific gravity = 2.8890.

3. The same, bluish, highly crystallised. Specific gravity = 2.76057.

4. The same, finer grained, containing thin layers of mica. Specific gravity = 2.7419.

5. The same, gray coloured, and the crystals larger. Specific gravity = 2.7302.

The mean specific gravity of these five specimens is = 2.76607.

Gran. Quartz.		Mic. and Calc. Schist.	
Numbers.	Specific Gravities.	Specific Gravities.	Numbers.
1	2.6435	3.0642	1
2	2.6437	2.9385	2
3	2.6296	3.0476	3
4	2.65367	2.7293	4
5	2.6460	2.7935	5
6	2.6269	2.7907	6
7	2.6274	2.7499	7
8	2.6109	2.7728	8
9	2.6296	2.71845	9
10	2.6576	2.7206	10
11	2.6656	2.7087	1
12	2.6452	2.8890	2
13	2.6387	2.76057	3
Mean	2.639876	2.7419	4
		2.7302	5
		2.81039	Mean

Mic.

Calc.

From the inspection of the preceding table, it is evident that the specimens relatively to their specific gravity may be divided into two classes sufficiently distinct from one another. The specimens of granular quartz are in specific gravity comprehended between 2.61 and 2.66, nearly, and the mean is 2.639876. The micaceous rocks, including the calcareous, are contained between the limit 2.7 and 3.06, the mean of all the 15 specimens being 2.81039. Now it happens fortunately, that these two classes of rocks distinguished by their specific gravity are also distinguished by their position, so that the line which separates them can be accurately traced out on the face of the mountain. As to the arrangement of the same two classes of rock in the interior of the mountain, there are only two different suppositions, as already observed, which possess any degree of probability, and the result of each is hereafter to be given. The curve line in the plan of the mountain divides the quartzy from the micaceous rocks.

I shall now proceed to state the principles on which the present investigation is founded, and the result to which it has led.

According to Dr. HUTTON's construction, if O (Fig. 1.) be the place of the plummet in the south observatory, ON the direction of the meridian, and if with a radius ON = 13333 feet, or $\frac{40000}{3}$, a quadrant of a circle be described, viz. WRN; if ON be divided into 20 equal parts, and if from O as a centre, through each of these points of division, circles be described: lastly, if through O, radii as OH, OG, &c. be drawn such that the sine of the angle which each of them makes with the meridian shall differ from the sine of that

which the contiguous radius makes with the meridian by $\frac{1}{12}$ of the radius; that is, if sine GON — sine HON. $= \frac{1}{12}$, &c. then shall every one of the twenty concentric rings be divided into twelve spaces, upon each of which if columns of homogeneous matter be supposed to stand, and to be of such altitudes as to subtend equal angles from O, the attraction of each column on the plummet at O, in the direction of the meridian ON, will be the same.

The attraction of any of these columns, as of that which stands on the base GHKL, is measured thus. Let $b =$ GL, the breadth of the column in the direction of the radius, $= \frac{40000}{3.12} = 666.666$ feet; $d =$ difference between the sines of the angles of azimuth, or sin. GON — sin. HON $= d$; E $=$ angle of elevation of the column above O: then the attraction $= b\,d \times$ sin. E.*

I have also used a theorem in these computations, which gives an accurate value of the attraction of a half cylinder of any altitude a, and any radius r, on a point in the centre of its base, and in the direction of a line bisecting the base. Let A be equal to that attraction; then A $= 2\,a$ Log. $\frac{r + \sqrt{a^2 + r^2}}{a}$, or A $= 2\,a$ Log. $\frac{r}{a}\left(1 + \sqrt{1 + \frac{a^2}{r^2}}\right)$.

Fig. 2. represents a vertical section of Schehallien in the direction of the meridian of the south observatory O.† The line QR represents the level of the lowest part of the base of

* Phil. Trans. Vol. LXVIII. p. 751.

† The observatories O and P are not in the same meridian; they are however nearly so; and the section through P in the direction of the meridian would not differ sensibly from that which is here given

the mountain. P is the north observatory; the part of the section coloured with a reddish brown represents the granular quartz, supposed here to constitute the interior as well as the summit of the mountain. The dark colour represents the schistus; the two belts of grey are the limestone strata on the north and south sides. OR or the elevation of the south observatory above the lowest part of the base of the mountain is 1440 feet.

Fig. 3. is a section of the mountain in the direction perpendicular to the meridian of the south observatory. This section, though not referred to in any of the computations, is useful for enabling one to form an idea of the structure and figure of the mountain.

Draw OL (Fig. 2.) parallel to the horizon. With OR as an axis, and with a radius of 13333 feet, suppose a cylinder to be described, and let it be cut into two semi-cylinders by a plane passing through OR perpendicular to RQ the meridian. Then the whole of the mountain on the north side of this plane disturbs the direction of the plummet by drawing it toward the north. But the part of the mountain to the north of this plane, and between the levels O and R is equal to one of the semi-cylinders above mentioned, *minus* the empty space between the surface of the ground and the horizontal plane passing through O. If therefore S denote the attraction of the semi-cylinder, and V that which the void space would have were each pillar in it to consist of matter of the same density with the part of the same pillar which is under the surface, S —V will represent the attraction or disturbing force of all that part of the mountain which is north of OR and under the level of O.

Again, putting S′ and V′ to express the same things for the part of the mountain to the south of O, the whole attraction of that part equal S′ — V′, and this acting in an opposite direction to the other, or tending to restore the plummet to its mean position, is to be subtracted from the former quantity, so that the whole disturbing force by which the part of the mountain below the level of O acts upon the plummet at O, is S — V — S′ + V′. To this the attraction of the upper part of the mountain, or that which is above the level of O, being, as it happens, wholly to the north is to be added, and if it be called T, the whole disturbance on the plummet at O is S — S′ —V + V′ + T.

In Dr. HUTTON's computation, S and S′, or the attraction of the half cylinders on opposite sides of O are equal to one another, the cylinder being supposed to consist of matter of the same density throughout; they must therefore destroy one another, and consequently, according to that hypothesis, they did not require to be calculated. The case here is not the same; for the matter in the two semi-cylinders not being of uniform density, nor having its inequalities similarly distributed, the attraction of each must be calculated in order that their difference, or S — S′ may be found.

If Σ, Σ′, U, U′, and T′ denote the same quantities for he observatory P on the north side of the mountain, then the disturbing force on the plummet at P, = Σ — Σ′ — U + U′ + T′; and so the whole force which alters the direction of gravity is S — S — V + V′ + T + Σ — Σ′ — U + U′ + T′.

The computation of these quantities for the columns in the quadrant north-west of O, will serve to explain the method followed in all the rest.

The whole cylinder of which OR is the axis being divided into 960 columns, the quarter of it must consist of 240, all of which, as far as their bases are concerned, are of equal force in attracting the plummet at O, so that the difference of their effects depends entirely on their altitude. Let O, (Fig. 1.) represent the south observatory, ON the meridian, and the quadrant ONW a horizontal section through O of one-fourth of the cylinder, on which the bases of the columns are marked as in the figure. Let *a b c* be the bounding line of the quartz projected on the plane of this section, the columns whose bases are within that line being supposed wholly of quartz, and those without it of micaceous schistus. If we suppose the columns that have their tops in this section to be extended downwards to the depth of 1440 feet, we shall have the quarter cylinder divided into 240 columns, that would be of equal disturbing forces, were they of equal density, and equal apparent depression below the point O. The inspection of the figure serves to distinguish the columns of quartz from those of micaceous schistus. In those columns which consist of both rocks, the proportion of the quartzy to the micaceous part could be judged of with sufficient accuracy by the eye. To assist the eye, however, the figure being first constructed to a large scale, I used to stretch a fine thread either in the direction of a radius passing through O, or in a line at right angles to that direction (according as the case seemed to require), so as to divide the quadrilateral into two quadrilaterals equal, as nearly as the eye could judge, to the irregular divisions made by the boundary of the quartz and schistus. The proportion of the parts was then easily ascertained. Now, by the first of the theorems laid down above, the attraction of

any column on the plummet at O, estimated in the direction of the meridian ON, if b be the breadth of it in the direction of the radius, d the difference of the sines of the azimuths of the two edges, E the angle which the length of the column subtends at O, if its density were $= 1$, would be $b\,d \times$ sin. E. But if the density of the rock be expressed by any other number, the attraction just found must be multiplied by that number in order to give A the real attraction of the column. Thus if Q denote the density of the granular quartz, and M that of the micaceous schistus, we have in the former case $A = b\,d$ Q. sin. E, and in the latter, $A = b\,d$ M. sin. E. In these formulas $b = 666.66$ feet, and $d = \frac{1}{12}$, by the construction already explained; therefore $A = (55.55)$ Q sin. E, or $= (55.55)$ M. sin. E.

The calculation of sin. E is very easy, for the length of each column or its depth below O being 1440 feet, and the middle of the first ring being 333.33 feet distant from O; of the second 1000, reckoning from O, if n be the number of any ring, the distance of its centre from O is $\frac{2n-1}{3} \times 1000$, so that tan. $E = \frac{1440}{\frac{2n-1}{3} \times 1000} = \frac{3 \times 1.44}{2n-1}$.

The sine corresponding to this tangent taken from the tables, and multiplied into 55.55, and the product into Q or M, will give the attraction of the column. Therefore to have the attraction of the ring of columns of the order n, the quantity now obtained must be multiplied by 12, that being the number of columns in one ring, having all by hypothesis the same altitude, so that the whole attraction of the ring $=$ (666.66) Q sin. E, &c.

The attraction of each of the twenty rings being thus computed, their sum gives the attraction of the quarter cylinder.*

From the projection of the columns in Fig. 1. it appears that the first six rings in the NW. quadrant are entirely of quartz, that the five following are mixed, being partly quartzy partly micaceous, and that the nine remaining columns are wholly micaceous. The little table that follows contains the proportions of quartzy and micaceous rock in the five rings just mentioned.

Sectors.

Rings.	1	2	3	4	5	6	7	8	9	10	11	12
7	$\frac{7}{10}q + \frac{3}{10}m$	$\frac{8}{10}q + \frac{2}{10}m$	$\frac{9}{10}q + \frac{1}{10}m$	q	q	q	q	q	q	q	q	q
8	m	m	m	$\frac{1}{6}q + \frac{5}{6}m$	$\frac{2}{3}q + \frac{1}{3}m$	$\frac{5}{6}q + \frac{1}{6}m$	q	q	q	q	q	q
9	m	m	m	m	m	m	$\frac{2}{3}q + \frac{1}{3}m$	q	q	q	q	$\frac{7}{11}q + \frac{4}{11}m$
10	m	m	m	m	m	m	m	$\frac{1}{2}q + \frac{1}{2}m$	$\frac{1}{2}q + \frac{1}{2}m$	q	q	$\frac{1}{2}q + \frac{1}{2}m$
11	m	m	m	m	m	m	m	m	m	$\frac{1}{2}q + \frac{1}{2}m$	$\frac{1}{2}q + \frac{1}{2}m$	m

This table is constructed only for that part of the north-west

* It was most convenient to compute the attraction of the quarter cylinder in this way, though merely an approximation, because the columns of which it consisted are not all of the same specific gravity. In the case of their being homogeneous, the attraction of the quarter cylinder might be computed *exactly* by the second theorems given above. Indeed I investigated that theorem for the purpose of examining into the degree of accuracy that this approximation actually possessed; and I had the satisfaction to find, that when the two methods were applied to the same half or quarter cylinder, (supposed homogeneous,) the difference of the results did not exceed a two thousandth part of the whole. This demonstrated in a very satisfactory manner the accuracy of the method pursued by Dr. Hutton.

quadrant in which columns occur of two different rocks; and a rectangular cell is assigned to each column in the five rings to which the table refers. The letters Q and M denote quartz and mica; and where one letter only occurs, the column is entirely of the rock which it denotes. In the cells where both letters occur, the column consists of both rocks in the proportion expressed by the fraction prefixed to each letter. Thus in the seventh ring, the first quadrilateral is $\frac{7}{10}$ quartz and $\frac{3}{10}$ mica; the second, $\frac{8}{10}$ quartz and $\frac{2}{10}$ mica; the third, $\frac{9}{10}$ quartz and $\frac{1}{10}$ mica; the remaining nine being entirely quartz.

Now to apply the tables thus constructed to the computation of the attraction of any of the quarter cylinders, it must be observed, that sin. E is to be found for any column in the way already explained, and is then to be multiplied by $b\,d$, b being $= \frac{2000}{3}$ and $d = \frac{1}{12}$, so that $b\,d = \frac{2000}{3 \times 12} = \frac{500}{9}$, and therefore the coefficient of Q or M is $\frac{500}{9} \times$ sin. E.

When the whole ring is of the same rock, the coefficient of sin. E computed for a single column is to be multiplied by 12, so that the whole attraction of the ring $= \frac{500}{9} \times 12 = \frac{2000}{3} =$ 666.66, as before determined.

In the mixed columns the sine of E is to be multiplied both into $b\,d$, and into the fraction prefixed to Q for the quartz, and to M for the mica; or if we would include the whole ring, as E is the same for all the columns contained in it, we must multiply $b\,d$ by the numbers denoting the proportion of quartz or of mica in the whole of that ring. Thus in ring 5, the first in the preceding table, the whole quartz = 11.4, the

sine of E being = .3157, and the attraction due to the quartz $= \frac{11400 \times Q}{3 \times 6} \times .3157 = (129.9423)\,Q$.

In this manner the attraction of the whole cylinder on the plummet at O is readily computed; but it muct be diminished on account of the part by which this cylinder rises above the surface of the ground. The quantity that is to be subtracted is computed from the sines of the depressions of the tops of the different columns below the observatory O; and Dr. HUTTON's paper either actually exhibits* those sines, or furnishes us with the means of readily computing them.† When a ring is wholly of the same species of rock, the sum of the sines of the depressions of all the columns in that ring is given in the tables, and needs only to be multiplied by $\frac{500}{9}$ to give the coefficient of Q or M as far as that ring is concerned.

Again, when in the same ring some columns are of quartz and others of mica, the sines of depression must be computed trigonometrically for each column by help of the data contained in the tables above referred to. The sum of those sines for the quartz columns being multiplied by $b\,d$ gives the coefficient of Q.

Where the same column is of two different kinds of rock, the sine of the depression, or of E, must be multiplied into $b\,d$, and divided in the proportion of the numbers prefixed to Q and M in the cell belonging to the column.

All this may be illustrated by the calculation of the attraction of the columns belonging to the above table. In the seventh ring the first columns are mixed, the next three are

* Phil. Trans. Vol. LXVIII. from p. 769 to 776.

† Ibid. from p. 759 to 765.

entirely of quartz, and the remaining six are wanting, that is to say, their tops are not depressed below the level of O, as may be seen in Table V. of Dr. HUTTON's paper. From that table it also appears, that the depth of the summit of the first column of the seventh ring below the level of O is 250 feet; of the second 240, of the third 200, of the fourth 150, of the fifth 60, and of the sixth 30. From these measures the angles of depression may be computed. Thus, if 250 be divided by the radius of this ring, viz. $\frac{13000}{3}$, we have .0577 for the tangent of the depression, or of E, and the sine which corresponds is .0568. As $\frac{7}{10}$ of this column consists of quartz, we must take $\frac{7}{10}$ of this sine for the proportional part of the coefficient of Q. In like manner, the sine of the depression of the top of the second column is .0545, of which taking $\frac{8}{10}$, we get .0436 for the part of the coefficient of Q belonging to this column. So also for the third ring, the proportional part of the sine is .04149. The fourth, fifth, and sixth columns being entirely of quartz, no proportional parts are to be taken; their sines, computed as before, are .0346, .0138, .0069; and the sum of all these six numbers is .18015.

Calculating in the same way for all the columns that are entirely or partly of quartz in the north-west quadrant, we have the amount of the whole = .2534. Now the total sum of the sines of the depressions in this quarter, is 13.534. (See Dr. HUTTON's computations, page 83). From this number, if .2534 be taken away, there will remain 13.2806 as the coefficient of M, arising from the depressions of the micaceous columns.

Now the sum of the sines belonging to the quartz in the

quarter cylinder itself has been found = 53.3532, from which taking away .2534, there remains .53.0998 for the entire sum of the sines belonging to the quartz under the level of O in the north-west quarter of the mountain.

In like manner the sum of the sines computed for the micaceous columns and parts of columns in the north-west quarter cylinder, is 23.0124, from which taking away 13.2806, the deficient or negative part, there remains 9.7318. The numbers thus found being multiplied by $\frac{500}{9}$, give the coefficients of Q and M for the north-west quarter of the mountain below the level of O, and make its attraction = (2949.99) Q + (540.655) M.

A similar computation being made for the quarter cylinder on the north-east of O, we have its attraction = (2974.299) Q + (577.98) M, to which adding their former attraction (2949.99) Q + (540.655) M, we have S—V, or the attraction of the mountain on the north side of O, and under the level of O = (5924.289) Q + (1118.635) M. In like manner the attraction of the south-west quadrant deduced partly from the quarter cylinder, and partly from Dr. HUTTON's calculations = (1049.18) Q + (1819.66) M, and of the south-east = (1567.894) Q + (1052.129) M; the sum of which, or S′ — V′, gives (2616.574) Q + (2871.789) M; to be subtracted from the former, in order that we may have the total disturbing force of the part of the mountain below O, which is therefore = (3307.715) Q — (1753.154) M.

Lastly T, or the attraction of the part of the mountain above O (which is on the north), when computed from the sums of the sines of the elevation of the columns above O, as given

by Dr. HUTTON, is found = (2474.889) Q + (150.855) M, which, added to the preceding, gives the whole attraction on the plummet at O = (5782.104) Q — (1903.209) M.

The same quantities calculated for P, the north observatory, are (8061.022) Q — (3127.05) M. To which adding the attraction just found for O, we have (13843.126) Q — (5030.214) M = the total force of attraction increasing the convergency of the plumb line on opposite sides of the mountain.

Now if D be the mean density of the globe, it follows from Dr. HUTTON's calculations that 87522720 × D is the measure of the attraction of the whole earth. But the Astronomer Royal having found by his observations, that the sum of the deviations of the plumb line on opposite sides of the mountain is 11.6 seconds, the attraction of the earth is therefore to the sum of the opposite attractions of Schehallien, as radius to the tangent of 11″.6, that is as 1 to .0000562839, or as 17781 to 1; or, making an allowance for the centrifugal force arising from the earth's rotation, as 17804 to 1. Therefore 17804 : 1 :: 87522720 × D : (13843.126) Q — (5030.214) M, so that $\frac{87522720}{17804}$ D = (13843.126) Q — (5030.214) M, and hence D = $\frac{13843126 \times Q - 5030214 \times M}{4915902}$, or D = (2.816) Q — (1.023) M.

If we suppose Q = 2.639876 and M = 2.81039, as in the table above, D = 4.55886.

Dr. HUTTON makes D = $\frac{17804}{9933}$ multiplied into 2.5, the supposed density of the rock,* which gives D = 4.481, considerably less than the preceding. If in the formula D = (2.816) Q — (1.023) M we make Q = M = 2.5, the result should

* Phil. Trans. Vol. LXVIII. p. 781.

agree with Dr. HUTTON's, and does so very nearly, making $D = 4.482$.

In all this, we have proceeded on the supposition that the granular quartz not only constitutes the summit of the mountain, or the part above the level of the observatories, but that it also descends into the interior of the mountain down to its base, where it is bounded by the curve line *abe* (Fig. 1). On the other supposition mentioned above, that the granular quartz does not constitute the interior nucleus of the mountain, but is confined to the upper part of it, the rest consisting of micaceous schistus, our formula, after undergoing certain changes, may also be accommodated to this hypothesis. In the value of the attraction of the part of the mountain below O, viz. $(3307.715)\,Q - (1753.154)\,M$, we must suppose $Q = M$, when the above quantity becomes $(1554.561)\,M$. To this we are to add T, or the attraction of the part of the mountain above O, which remains the same as before, viz. $(2474.389)\,Q - (150.055)\,M$, to which if we add $(1554.561)\,M$, the sum $(2474.389)\,Q + (1404.506)\,M$ is the whole attraction on the plummet at O, according to this new hypothesis.

If the same changes are made with respect to the observatory P, we shall have the total attraction. Now the attraction of the part of the mountain below P $= (5593.347)\,Q - (3172.15)\,M$, which if $Q = M$ becomes $(2421.197)\,M$. Also the attraction of the part above P is $(2467.675)\,Q + (45.15)\,M$. If to this be added $(2421.197)\,M$ the amount, or $(2467.675)\,Q + (2466.347)\,M$ is the total attraction on the plummet at P.

To the total attraction at O $= (2474.389)\,Q + (1404.506)$

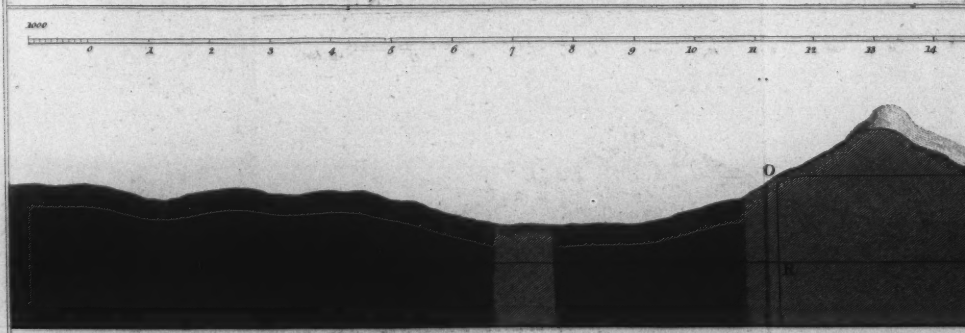
M, add the total attraction at P = (2467.675) Q + (2466.347) M; then the total attraction by which the direction of gravity is altered by the mountain, is (4942.064) Q + (3870.853) M. Hence as before $D = \frac{(4942.064)\,Q + (3870.853)\,M.}{4915.902.}$ or D = (1.0053) Q + (0.78743) M.

Here if we make as before Q = 2.639876 and M = 2.81039, we shall have D = 4.866997. This therefore is the mean density of the earth, on the supposition that the interior of Schehallien, on a lower level than the observatories, consists of micaceous schistus. The measure thus obtained, for the mean density or mean specific gravity of the earth, is above that of any of the precious stones, and is nearly a mean between the results of Dr. HUTTON and Mr. CAVENDISH. According to the former, D = 4.481; according to the latter, D = 5.48, the mean of which is 4.98. The difference between this and the last of our results is nearly = .1, or less than a forty-fifth part.

If we are to consider the experiments on Schehallien singly, it seems highly probable that the mean density of the earth is contained between the limits deduced from the two different suppositions concerning the structure of the mountain, so that it cannot be less than 4.5588, nor greater than 4.867. The mean of these is nearly 4.713.

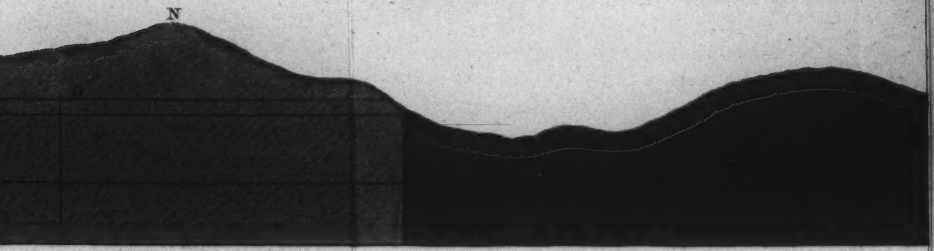
It is however desirable that an element so important in physical astronomy, as the mean density of the earth, should be the result of many experiments. The principle on which those at Schehallien were made seems the most likely to lead to accurate conclusions. In the selection of the places fit for such observations, the homogeniety of the rock is a condition

Longitudinal section of Schehallien through the two Co



Section of Schehallien in the plane of th

Fig. 1.



wo Cairns N & K; bearing from the Meridian N. 85. 18 ¼ W.

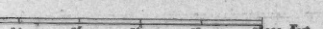
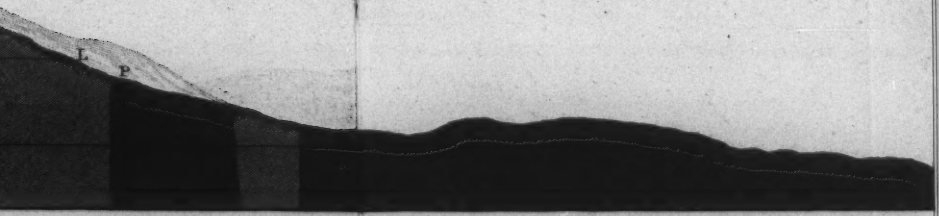


Fig. 2.



of the Meridian of O the South Observatory.

Engraved by J.ᵉ Basire.

that merits particular attention, and is hardly to be looked for any where but among granite mountains, as they alone afford a perfect security that their interior and exterior are composed of the same materials. Granite is the lowest of the rocks, and whenever it appears at the surface we may be assured, that on penetrating deeper, we shall meet with no other.

It is therefore to the primitive mountains, and among them to the granitic, that such experiments as those made at Schehallien ought to be confined. The want of homogeneity will then be on the outside of the mountain only, and can easily be estimated. The granite may be covered at the bottom of the mountain and even to a considerable height on its sides with beds of gneiss, mica slate, hornblende slate, &c., the quantity and position of which can easily be ascertained by observation.

XIX. *Observations and Experiments on Vision.* By William Charles Wells, *M. D. F. R. S.*

Read July 4th, 1811.

1. I was consulted, in the beginning of the year 1809, upon a disease of vision, which, as far as I know, has not hitherto been mentioned by any author. The subject of it was a gentleman about thirty-five years old, very tall, and inclining to be corpulent. About a month before I saw him, he had been attacked with a catarrh, and as this was leaving him, he was seized with a slight stupor, and a feeling of weight in his forehead. He began at the same time to see less distinctly than formerly with his right eye, and to lose the power of moving its upper lid. The pupil of the same eye was now also observed to be much dilated. In a few days, the left eye became similarly affected with the right, but in a less degree. Such was the account of the case, which I received from the patient himself, and from the surgeon who attended him. The former added, that previously to his present ailment his sight had always been so good, that he had never used glasses of any kind to improve it. On examining his eyes myself, I could not discover in them any other appearance of disease, than that their pupils, the right particularly, were much too large, and that their size was little affected by the quantity of light which passed through them. At first, I thought that their dilatation was occasioned by a defect of sensibility in the

retinas; but I was quickly obliged to abandon this opinion, as the patient assured me, that his sensation of light was as strong, as it had ever been during any former period of his life. I next inquired, whether objects at different distances appeared to him equally distinct. He answered, that he saw distant objects accurately, and in proof told me what the hour was, by a remote public clock; but he added, that the letters of a book seemed to him so confused, that it was with difficulty he could make out the words which they composed. He was now desired to look at a page of a printed book through spectacles with convex glasses. He did so, and found that he could read it with ease. From these circumstances it was very plain, that this gentleman, at the same time that his pupils had become dilated, and his upper eye-lids paralytic, had acquired the sight of an old man, by losing suddenly the command of the muscles, by which the eye is enabled to see near objects distinctly; it being known to those, who are conversant with the facts relating to human vision, that the eye in its relaxed state is fitted for distant objects, and that the seeing of near objects accurately is dependant upon muscular exertion.

The disease of which I have spoken is perhaps not extremely rare. For having related the preceding instance of it to Mr. Ware, a Fellow of this Society, he was kind enough shortly after to send to me a young woman, who appeared to be likewise affected with it. But as I saw her only once, and had not then sufficient time to examine her case minutely, I speak with diffidence concerning its nature.

II. After I had reflected frequently upon these cases, it occurred to me, that, as the juice of the herb Belladonna, when applied to the eye, occasions the pupil to dilate considerably,

and to become unalterable by light, an effect might at the same time be produced by it upon vision, similar to that which I have just described. I had, indeed, in the course of a few years immediately preceding, applied Belladonna several times to my own eyes, without observing any change in my sight, beyond what I referred to the increased size of the pupils; but as I had not looked for any other, I thought it possible, that some additional one might have happened, without my having perceived it. I resolved therefore to make the experiment anew. But to conduct it with precision, it was previously necessary to know, to what extent I possessed the faculty of adapting my eyes to different distances. On this subject I had made many experiments with great care, nearly twenty years before, and had ascertained,* that with my left eye, which was more perfect than the right, I could bring to single points on the retina pencils of rays, which flowed from every distance, greater than that of seven inches from the cornea. In the mean time, however, my eyes had altered considerably, with respect to their seeing near objects distinctly, and I had, in consequence, been obliged, not only to use convex glasses, but to change them several times for others of higher power. No dependance therefore being now to be placed in my former experiments, in regard to the present state of my sight, I repeated them, and found, to my great surprise, that the power I once possessed of adapting my eyes to different distances was entirely gone; in other words, that I was now obliged to regard all objects, whether near or remote, in the same refractive state of those organs. I found also, that my eyes, considered as mere optical instru-

* Essay on Single Vision with two eyes, &c. p. 137.

ments, were nearly the same as they had been in my youth, and that the convex glasses which I used did very little more, than supply, with respect to near objects, the place of a living power which I had lost, without compensating, except in a very small degree, for any alteration in the external shape of the eye, or any change in the configuration of its interior parts. I ascertained, for instance, that to give my left eye the refractive power which it formerly possessed while in its most relaxed state, that by which it was enabled to bring a pencil of parallel rays to a point on the retina, a glass of thirty-six inches focus was fully sufficient; whereas to produce an equal effect upon rays proceeding from a point at the distance of seven inches from my eye, the other extremity of my ancient range of perfect vision, I was now obliged to employ a glass having a focus of only six inches. I regret much, that I had not made such experiments frequently before, as I think it very probable, that I should have found a period in the progress of my vision to its present state, in which my capacity of seeing distant objects was the same as in my youth, and when therefore the whole of my imperfect vision of near objects would have been owing to a loss of the muscular powers of my eye.

As there can be no good reason for supposing, that the changes which have occurred in my eyes are different from those, which the eyes of by far the greater number of persons, who are not short-sighted, undergo at the approach of old age, it is evident, that the experiments of Dr. YOUNG* on the eye of HANSON, whom the learned author considered as a very fair subject for such trials, furnish no proof, that the

* Phil. Trans. 1801, p. 66.

want of the crystalline lens disables a person from having perfect vision at different distances; for as HANSON was sixty three years old, it is highly probable, that the results of the experiments would have been exactly the same, if he had still possessed that part of his eye.

III. Having discovered that my own eyes were unfit for the experiments, which I wished to be made with Belladonna, I instructed an ingenious young physician, Dr. CUTTING, from the island of Barbadoes, and now residing there, in the manner elsewhere described by me,* of ascertaining his range of perfect vision by means of luminous points. This he found, in consequence, to begin, with respect to his left eye, at the distance of six inches, and not to terminate at the distance of eight feet, beyond which he could not see clearly the object, with which he had hitherto made his experiments, the image of the flame of a candle in the bulb of a small thermometer. The flame of a lamp, distant about sixty yards, gave a faint indication of its rays meeting before they fell upon the retina; the rays from a star had very evidently their focus a little before that membrane. He now applied the juice of Belladonna to his left eye. Half an hour after, when his pupil was but little dilated, perfect vision commenced at the distance of seven inches; in fifteen minutes more, it began at the distance of three feet and a half. When his pupil had acquired its greatest enlargement, the rays from the image of the flame of a candle, in the bulb of a small thermometer at the distance of eight feet, could not be prevented from converging to a point behind the retina. The rays from lamps still more distant, and from stars, had their focuses at the same time on

* Essay on Single Vision, &c. p. 116.

the retina. This state of vision continued, in its greatest extent, to the following day; and it was not till the ninth day after the application of the Belladonna, that he completely recovered the power of adapting his eye to near objects. While his left eye was thus affected, the vision of the right remained unaltered.

Dr. CUTTING remarked, while his left eye was returning to its natural condition, that the diminution of the pupil, and the increase of the range of perfect vision, did not keep regular pace with each other; but that after his pupil had nearly returned to its former size, his capacity of adapting the eye to different distances was still very limited. As these effects therefore are not inseparably connected, they may occur in others in a different manner from that which he observed. A great degree of dilatation, for example, may take place in the pupil, without a total want of the power to adapt the eye to different distances.

Though I could not doubt the accuracy of Dr. CUTTING's observations, more especially as the altered state of his eye had lasted a considerable time, and as he had not been prevented by other occupations from attending minutely to the appearances, which were consequent upon it; yet, as he was the first person who had ever applied Belladonna to his eye for the purpose which has been mentioned, and as the results had been remarkable, I requested him to repeat the experiment with his other eye. He complied with my desire, and found, that the appearances which followed were similar to those, which had been produced by the application of Belladonna to his left eye.

It will, perhaps, be thought extraordinary, that Dr.

CUTTING's eye in its relaxed state, before the application of the Belladonna, brought parallel rays to a focus anterior to the retina; but that similar rays met in a point upon the retina, while the eye was under the full influence of that substance; as it may hence seem, that the Belladonna had done more than merely suspend the exercise of the power, by which the eye is fitted to see near objects distinctly. An observation drawn from the former state of my own sight will, I expect, make this matter plain.

When I enjoyed the faculty of adapting my eyes to objects at different distances, the rays of a star, which was viewed attentively by me, always met in a point a little before the retina;* whence I at first concluded, that my eye was unfit for accurate vision by parallel rays. But I afterwards found, that if I looked at a star carelessly, its rays had then their concourse on the retina. In the former case, from long habit, originating in my having chiefly viewed near objects with attention, some small exertion was made for the accurate view of a distant object, though none was requisite; in the latter, all demand for exertion ceasing, my eye fell into the most relaxed condition, that by which it was fitted for parallel rays. Dr. CUTTING's eye seems to have been similar to what my own once was, in regard to such rays; but as he had not acquired the faculty of viewing a distant object, without making some exertion, the rays from a star crossed one another in his eye before they came to the retina. The capacity, however, of making any exertion was taken away by the Belladonna, and pencils of parallel rays were, in consequence, brought to points upon that membrane.

* Essay on Single Vision, &c. p. 138.

IV. Being now in possession of a new instrument, I next attempted to gain, by means of it, some illustration of the changes, which the vision of short-sighted persons undergoes from age.

It has been very generally, if not universally, asserted by systematic writers upon vision, that the short-sighted are rendered by age fitter for seeing distant objects than they were in their youth. But this opinion appears to me unfounded in fact, and to rest altogether upon a false analogy. If those who possess ordinary vision, when young, become, from the flatness of the cornea, or other changes in the mere structure of the eye, long-sighted as they approach to old age, it follows, that the short-sighted must, from similar changes, become better fitted to see distant objects. Such appears to have been their reasoning. But the course pursued by nature seems very different from that which they have assigned to her. For of four short-sighted persons of my acquaintance, the ages of whom are between fifty-four and sixty years, and into the state of whose vision I have inquired particularly, two have not observed that their vision has changed since they were young, and two have lately become, in respect to distant objects, more short-sighted than they were formerly. As the manner, in which this change has occurred, is unnoticed, I believe, by any preceding author, I shall here relate the more remarkable of the two cases.

A gentleman, who is a Fellow of this Society, became short-sighted in early life, and as his profession obliged him to attend very much to minute visible objects, he for many years wore spectacles with concave glasses almost constantly, by the aid of which he saw as distinctly, and at as great a variety

of distances, as those who enjoy the most perfect vision. At the age of fifty, however, he began to observe, that distant objects, though viewed through his glasses, appeared indistinct, and he was hence led to fear, that his eyes were affected with some disease. But happening one day to take up, in an optician's shop, a single concave glass, and to hold it before one of his eyes, while his spectacles were on, he found to his great joy, that he had regained distinct vision of distant objects. With regard to such objects, therefore, he had lately become shorter-sighted than he had formerly been. But along with this change, another occurred of a directly opposite kind. For when he wished to examine a minute object attentively, such as he used to see accurately by means of his spectacles, he now found it necessary to lay them aside, and to employ his naked eye. He had become, therefore, in respect to near objects longer-sighted. The power, consequently, in this gentleman, to adapt the eye to different distances, is either totally lost or much diminished; but the point, or small space to which his perfect vision is now confined, instead of being the most remote to which he could formerly accommodate his eyes, as is commonly the case with the ordinarily sighted when they are becoming old, is now placed *between* the two extremes of his former range of accurate vision. The eyes of the other short-sighted person, a physician of considerable learning, whose vision has been altered by age, have been affected in a similar manner, but not in so great a degree.

As the only change, which had occurred from age in the sight of such of my acquaintance as were considerably myopic was a lessening, on both sides, of their range of perfect vision, I conceived that this might be the ordinary procedure of

nature in such cases, and that it might be imitated, in a young short-sighted person, by the application of Belladonna to his eyes. I have hitherto not been able to obtain permission to make the experiment on any young person, who is very short-sighted. Two gentlemen, however, who are somewhat short-sighted, have readily submitted to it; one of them, Mr. BLUNDELL, a diligent and ingenious student of medicine; the other, Mr. PATRICK, a well educated young surgeon in London. The first experiment was on Mr. BLUNDELL, and the apparent result was, that the range of his accurate vision was considerably diminished at both ends, but not annihilated. Mr. BLUNDELL, however, afterwards informed me, that he repeated the experiment with more care in the country, and found, that in one eye the nearest point of perfect vision was moved forward about two-thirds of the whole range, and in the other about one-third; but that, with respect to both eyes, the most remote points of the ranges were unchanged. He added, that while one eye was under the influence of the Belladonna, the other became shorter-sighted than it had been before; but the difference was not so great, as to induce me to place entire confidence in the justness of his observation. I think it right to mention here, that from mistake I applied only two-thirds of the ordinary quantity of Belladonna to his eye, in the first experiment; and that he probably, in consequence of my example, applied no more when he made the second; as this might have been the reason, that during both experiments he retained, in part, the capacity of adapting his eyes to different distances.

The experiment on Mr. PATRICK was conducted by myself, after he had been frequently exercised in observing the extent

of his perfect vision. The results were similar to those which had been remarked by Dr. CUTTING. The power of altering the adaptation of his eye, according to the distance of the objects viewed, was for some time entirely lost, and his sight became accurately fitted for such only, as were placed at the farther extremity of his former range of perfect vision. While one eye was under the influence of the Belladonna, the vision of the other was unaffected.

From these experiments it seems probable, that Belladonna will in no case produce the same effect upon a young short-sighted person, that age has produced in the two instances of which I have spoken. I expect, however, to have an opportunity of repeating the experiment on two persons, who are very considerably short-sighted, and I shall take the liberty of communicating the result to the Royal Society, together with some observations I have already made, and others which I hope to make, respecting those persons, who seem to retain to extreme old age the power of seeing perfectly, as far as the accommodating power of the eye is concerned, both distant and near objects; and of others, who, after being without this power for many years, appear to regain it at a similar period of life. Probably the making known my intention may facilitate its accomplishment, by inducing other Fellows of the Society to furnish me with opportunities of increasing my knowledge of these subjects. In the mean time, I shall offer a few words upon two other topics in vision, which seem to derive illustration from my experiments with Belladonna.

V. 1. Not only do the pupils move together, when both eyes are in a healthy state, but the pupil of one eye affected with gutta serena moves with the pupil of the other, as long as this

remains sound. These facts are generally, but in my opinion erroneously, attributed to an immediate sympathy between the pupils. For when the pupil of one eye becomes dilated from the application of Belladonna, the pupil of the other, so far from dilating, becomes smaller. It follows, therefore, that the size of the pupil is dependant, not only on the impression of light on the retina of its own eye, but on that also which is made on the retina of the other, and that the moving of the two together, which for the most part takes place, is only an accidental consequence of the fact which I have mentioned.

2. As the action of the external muscles of the eye has been frequently resorted to, for an explanation of its capacity to see objects perfectly at different distances, I requested Dr. CUTTING to attend to this matter. He accordingly ascertained, while his eye was in its natural state, the distance from his face of the nearest point, at which he could make the two optic axes meet, this being the greatest trial of strength, to which those muscles can be exposed. Shortly after, he repeated the experiments, while, in consequence of the application of Belladonna, he was without the power of adapting his eye to different distances, and found, that the strength of those muscles was not diminished. It follows, therefore, not only that the external muscles have little or no concern in fitting the eye to see distinctly at different distances, but that the same is true with respect to the cornea, as we cannot suppose, that its mechanical properties were altered by the Belladonna, or at least, that it became more inflexible from the application to it of the juice of that herb. I had before made a similar experiment on myself, by comparing what had been the strength of the external muscles of my eyes twenty years

ago,* with what it was after I had lost the power of altering their refractive state; but though I found no difference, yet, as their coats might have in the mean time become more rigid, I thought it right to have the experiment repeated, in a manner to which no objection could be taken.

The only other part of the eye, or its appendages, which remains for enabling us to see equally well at very different distances, is the crystalline; and that it does produce this effect, either wholly, or very nearly so, is manifest, from the necessity even young persons are under, who have lost it, of using glasses of very different convexities for near and remote objects. But in what way this important office is performed by it seems still unknown. The learned Dr. YOUNG, indeed, as well as others before him, has supposed, that the crystalline has the power of altering its figure; but the proofs hitherto given in favour of this opinion appear very defective. In 1794, I attempted to submit its justness to the test of direct experiments, by applying to the crystallines of oxen, which had been felled from thirty seconds to a minute before, chemical and mechanical stimuli, and those of Galvanism and electricity; but in no instance was any alteration of figure, or other indication of muscular power, observed. All of these stimuli were applied to the crystalline while it was surrounded by air, and some of them while it was covered with warm water. Last summer, after I knew that men lose, from increase of years, the faculty of altering the refractive state of the eye, I thought it possible, that the oxen on which I had made the experiments were too old for them. I therefore repeated most of them on the crystallines of a calf and a lamb; but

* Essay on Single Vision, &c. p. 136.

still no motion was to be seen. Dr. YOUNG has made similar experiments with a similar event; but he thinks that no argument can hence be derived against his opinion, as neither can motion be excited in the uvea, by any artificial stimulus. In the first place, however, it is not agreeable to just reasoning to regard an unknown thing as an exception to a general rule, rather than as an example of it; in the second, the motions of the uvea are involuntary, whereas the adaptation of the eye is, in part at least, under the command of the will; and in the third, the crystalline seems very unfit for performing the motions which he assigns to it; for if its figure be altered out of the body, by external force, it does not restore itself, but retains the shape which has been given to it, like a piece of dough, or soft clay. Possibly further experiments with Belladonna may contribute to remove the obscurity, which at present surrounds this subject.

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1811.		
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PRESENTS.		DONORS.
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INDEX

TO THE

PHILOSOPHICAL TRANSACTIONS

FOR THE YEAR 1811.

A

page

Aconite, juice of the leaves of, its effects on the animal œconomy, - - - - - 185, 193
Affection, nervous, on a case of, cured by pressure of the carotids; with some phisiological remarks, - - 89
Alcohol, its effects on the animal œconomy, - - - 179
—— exists in wine ready formed, - - - 339
Almonds, bitter, essential oil of, its extraordinary effects on the animal œconomy, - - - 183, 193
Arteries, carotid, effects of pressure on them - - 94

B

Baily, Francis, Esq. On the solar eclipse which is said to have been predicted by Thales, - - - 220
Belladonna, juice of, its effects on the eye - - - 380
Birds, small intestines of, an account of an appendix to them, 257
Brande, Wm. Thomas, Esq. His account of a vegetable wax from Brazil, - - - - - 261
—— His experiments to ascertain the state in which spirit exists in fermented liquors: with a table exhibiting the relative proportion of pure alcohol contained in several kinds of wine and some other liquors 337
Brodie, B. C. Esq. His Croonian Lecture, on some phisiological researches, respecting the influence of the brain on the action of the heart, and on the generation of animal heat, - - - - - - 36

page

BRODIE, B. C. Esq. Experiments and observations on the different modes in which death is produced by certain vegetable poisons - - - - - 178

C

Calomel, analysis of, - - - - - - 26
CAVENDISH, Mr. His experiments on the density of the earth referred to, - - - - - 376
Chlorine or *Chloric* gas, a name given to oxymuriatic acid, 32
Corrosive Sublimate, analysis of - - - - 26
CRUICKSHANK, Mr. His experiments on the serum of diabetic persons noticed - - - - 97
CUTTING, Dr. His observations on the effects produced on the eyes by the application of belladonna, noticed - 384

D

DAVY, HUMPHRY, Esq. LL.D. His Bakerian Lecture. On some of the combinations of oxymuriatic gas and oxygene, and on the chemical relations of these principles to inflammable bodies - - - - - - 1
——————— On a combination of oxymuriatic gas and oxygene gas, - - - - - 155
DAVY, Mr. E. His analysis of hyperoxymuriate of potash noticed, - - - - - 18
Derbyshire, an account of the great denudation of, - - 242
Duct, thoracic, not the organ through which fluids pass to the circulation of the blood, - - - - 165
DU HAMEL. His experiments on trees noticed, - - 210

E

Earth, mean density of, deduced from experiments on Schehallien, - - - - - - 376
Earths, metals of, on their combinations with oxygene and oxymuriatic gas, - - - - - 19
Eclipse, Solar, on that said to have been predicted by Thales, 220
Euchlorine, or *Euchloric* gas, a name given to a combination oxymuriatic gas and oxygene, - - - 162
——————— properties of - - 156

INDEX.

F

page

FAREY, MR. J. Senr. His account of the great Derbyshire denudation, - - - - - - - 242

FABRONI. His experiments on alcohol referred to, - - 337

Fluids, experiments to prove that they pass directly from the stomach to the circulation of the blood, and from thence into the cells of the spleen, the gall bladder, and urinary bladder, without going through the thoracic duct, 163

FONTANA, ABBE. His experiments on the ticunas noticed 201

G

Guinea Pigs, effects of woorara on them, - - - 194

H

HELLINS, REV. JOHN, B. D. On the rectification of the hyperbola by means of two ellipses, &c. Also some further observations on the same subject; being an Appendix to his paper inserted in the Phil. Trans. for 1802 - - 110

HERSCHEL, WILLIAM, LL. D. His astronomical observations relating to the construction of the heavens, arranged for the purpose of a critical examination, the result of which appears to throw some new light upon the organization of the celestial bodies - - - - - - 269

HERODOTUS, his account of the solar eclipse referred to 220

HOME, EVERARD, ESQ. His experiments to prove that fluids pass directly from the stomach to the circulation of the blood, and from thence into the cells of the spleen, the gall bladder, and urinary bladder, without going through the thoracic duct, - - - - - - - 163

HUMBOLDT, his account of a vegetable wax referred to 262

HUTTON, DR. His calculations on the attraction of Schehallien referred to, - - - - 357, 363, 371

Hyperbola, on the ratification of, by means of two ellipses, &c. Also some further observations on the same subject. Being an Appendix to a paper on the rectification of the hyperbola inserted in the Phil. Trans. for 1802, - 110

K

KNIGHT, THOMAS, ESQ. On the expansion of any functions of multinomials, - - - - - - - 49

KNIGHT, T. A. ESQ. On the causes which influence the direction of the growth of roots, - - - - 209

L

Landen, Mr. John, his memoir on the hyperbola and ellipses, referred to, - - - - - 111
Lecture, the Bakerian, - - - - - - - 1
——— the Croonian, - - - - - - 36
——— Addition to it for the year 1810, - - - 207
Liquors, Spirituous, the quantity of alcohol they contain, 346
Lussac, Gay, and Thenard, their discovery of the peroxides of potassium and sodium noticed, - - - 4

M

Macartney, James, Esq. His account of an Appendix to the small intestines of birds, - - - 257
Marcet, Dr. His letter to Dr. Wollaston on the subject of diabetes, - - - - 106
——————— His analysis of muriate of soda referred to 16, 174
Maskelyne, Dr. His astronomical observations on the mountain of Schehallien referred to, - - - 347
Metals, common. On their combinations with oxygene and oxymuriatic gas, - - - - - 22
Minium, native, addition to the account of it, inserted in the Philosophical Transactions for 1806, - - - 177
Multinomials, on the expansion of any functions of, - 49

N

Nebulæ, of such as have a cometary appearance - 306
——— of planetary, - - - - - 317
——— of stellar, - - - - - - 325
Nebulosities, observations of, never before published, 273

O

Observations, Astronomical, relating to the construction of the heavens, arranged for the purpose of a critical examination, the result of which appears to throw some new light upon the organization of the celestial bodies, - - 269
Oxides, metallic, on the action of oxymuriatic gas on them, 25
Oxygene, on its combinations with the metals from the fixed alkalies, - - - - - - 2

INDEX.

page

Oxymuriatic gas, on its combinations with the metals from the fixed alkalies, - - - - - 2
———— does not affect vegetable colours when dry, 29
———— does not crystallize at a low temperature, 30
———— impropriety of its name - - 32
Oxymuriatic compounds, some reflections on the nomenclature of, *ib.*

P

Parry, C. H., M. D. On a case of nervous affection cured by pressure of the carotids; with some physiological remarks, - - - - - 89
Presents received by the Royal Society, from November 1810 to July 1811, - - - - - 393
Playfair, John, Esq. His account of a lithological survey of Schehallien, made in order to determine the specific gravity of the rocks which compose that mountain, 347
Poisons, vegetable, experiments and observations on the different modes in which death is procured by them, - 178
———— experiments with, applied to the tongue or alimentary canal, - - - - - 179
———— experiments with, applied to wounded surfaces, - - - - - - 193

R

Roots, on the causes which influence their direction and growth, - - - - - - 209

S

Schehallien, account of a lithological survey of it, - 347
———— catalogue of specimens from, - - 358
Seymour, Lord Webb. His co-operation with Mr. Playfair in a lithological survey of Schehallien, - 348
Smithson, James, Esq. On the composition of zeolite 171
Sodium, an easy mode of obtaining it, - - - 15
Spirit, experiments to ascertain the state in which it exists in fermented liquors, - - - - - 337
Spleen, not the peculiar organ by which fluids pass directly from the stomach to the kidneys, - - - 169
Sugar, on the non-existence of, in the blood of persons labouring under diabetes mellitus, - - - - 96

INDEX.

T

Tobacco, the infusion of, its effects on the animal œconomy, page 186
———, *empyreumatic oil of*, its effects on the animal œconomy, 191

U

Upas antiar, its effects on the animal œconomy, - 196

V

Vision, observations and experiments on, - - 379

W

Wax, vegetable, an account of one from Brazil, - - 261
———— its properties, - - - - 262
Wells, Charles, M. D. His observations and experiments on vision, - - - - - - 379
Wines, quantity of alcohol they contain - - 345
Wollaston, W. H. M. D. On the non-existence of sugar in the blood of persons labouring under diabetes mellitus, 96
Woorara, its effects on the animal œconomy - - 194

Y

Young, Dr. T. His experiments on vision referred to, 382

Z

Zeolite, on its composition - - - - 171

From the Press of
W. BULMER & Co.
Cleveland-Row, St. James's,
London.